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NOTES AND COMMENTS.

Organic Life on Mars.

THE hold taken upon the popular mind by the astonishing revelations of astronomy is again in evidence. The planet Mars is the nearest neighbour to the earth, and at present the two are at the nearest parts of their circling courses. From the observatory at Nice, Mr. Javelle has seen on the southern edge of Mars a luminous projection. In accordance with the admirable co-operation among the astronomers all over the world, this interesting observation has been telegraphed from station to station. These telegrams naturally have found their way to the public Press, and from their reaction upon the journalistic mind have come a crop of articles and interviews, of speculations and theories, and all these turn upon the twin questions —is the planet Mars inhabited? and are the inhabitants signalling to us?

We share with everyone else the compelling fascination of these questions. Time was when the earth, like a little child, thought itself the centre of all things: now it has grown up enough to know that it is the tiniest unit in the ordered polity of the universe. It is an insignificant appanage of a system which is the merest dot in star-sown space. In the effort to reach some cognisance of our place in the unknown, the planet Mars seems to offer some hope of a standard of comparison, some basis for induction. With a knowledge only of this earth, the imagination may run riot and people the planets of this and other systems with benign or evil figments of fancy. With a knowledge, however slight, of the actual state of things on the surface of Mars, we should lose the morbid fictions engendered by isolation and at the least gain a method by which speculation might be guided.

It is now known that the physical conditions here and on Mars

are similar in many respects. Both the earth and Mars "turn alternate cheeks to the reverberation of the same blazing world," and both encounter the same, or nearly the same, range of temperature. Mars, like the earth, is covered by land and by sea. It has mountain ranges and volcanoes, plains and valleys; it has wind storms and rain and clouds. The polar regions are covered with snow and ice. Summer and winter succeed each other as here, although the actual transition from season to season is much more sudden. Mr. Norman Lockyer, for instance, says that he has seen a tract of snow, that would reach from London to Madrid, melt in a week, and suggests that the so-called canals of Mars are deep channels hollowed out by the sudden periodical melting of the snows. We know, of course, that the same chemical elements, and many of the compounds with which we are familiar here, exist on Mars. The only known physical condition differing much is the force of gravity, which is considerably less on Mars.

Can we conclude anything from these data about the presence of organic life? In the first place, organic life, as we know it, is associated invariably with protoplasm. In that mixture of materials, the chief chemical compounds present are the albuminous substances known as proteids. A temperature about freezing point arrests the activity of these: a temperature above 40 degrees centigrade destroys them. So far there is no difficulty, for certainly there are on Mars, as here, some parts where the temperature remains all the year round within these limits. Next, water must be present, and in this respect also, the conditions on Mars almost certainly are favourable. The water must be tolerably pure: the presence of a certain amount of salts would be no barrier, for, although even the best elementary text-books of biology know only the amœbæ of fresh water, amœbæ are common animals of brackish water, and of the sea-shore. The presence of free mineral acids in solution would be fatal: but from the similar physical and chemical conditions here and on Mars, there is every reason to believe that free acids would exist as seldom in nature there as here. The different conditions of gravity afford more perplexing problems. Oscar Hertwig and others, by experiments upon developing ova in centrifugal machines, and so forth, have shown that neutralising the effects of gravity makes little difference to the forces of growth. But the researches of Bütschli open up graver questions. If there be truth in his analogy between protoplasm and physical foams, only those who have attempted to repeat his experiments can judge how small are the differences in tensions and consistencies that make or mar the results. If the structures and movements of protoplasm depend, as he thinks, chiefly upon differences of surface tension, no one can say how a difference in the force of gravity would interfere with the delicate interaction of forces upon which tensions depend.

But granted the possibility of the mere existence of protoplasm

on Mars, a much wider set of problems at once arises. For we are more interested in the possibility of the existence of that reasoning intelligence which, within our experience, is always associated with such integrations of protoplasmic structures as we find in man and the higher animals. The framework and structure of all animals and plants are adapted so intimately to the existing conditions of gravity, that the course of the evolution of life under other conditions must elude even our imagination. If from the same simple beginnings a chain of life has grown up upon Mars, it must have grown up so different that to us its final results would be a horror and a monstrous prodigy. Even if it has grown so that its highest result has like us become able to be conscious of a side or part of the absolute truth of things, it is very improbable that this consciousness, acting in terms of and through different sense organs, possibly upon a different side of the absolute truth, could ever come into relation with ours. If ever we could "open up communication with Mars" in the sense of projecting such physical signs as would be intelligible to minds acting through sense organs like our own, it is more than probable that they would be unintelligible to minds deaf to what we hear, blind to what we see, not feeling what we touch, and realising their environment in ways inscrutable to us.

Lord Salisbury's Address.

The University of Oxford and the English nation have equal reason to be proud of the remarkable address of the President of the British Association; for, so far as the University is concerned, their supreme officer, the Chancellor, is chosen for very different reasons than his eminence in science; so far as the nation is concerned, Lord Salisbury is honoured as a man of affairs, as one who gained worthily and held worthily the great office of her Chief Minister. It is the fashion to say that the University is the home of effete culture; it is the fashion to say that politics are in the hands of mere rhetoricians, of shallow devotees of expediency. Here is the head of a great University, an acknowledged leader of politicians, who can hold enthralled on scientific subjects an audience as full of scientific experts as ever is gathered together. Discount all one may for the modulations of the practised speaker, for the political habit of exquisite adaptation to an audience, it remains to be said that unless Lord Salisbury had had solid scientific matter in his speech, as well as subtle intellectual form, he would not have received the enthusiastic reception that was his.

It is easy to analyse his arguments and to criticise his conclusions. To these agreeable tasks we shall presently address ourselves. It is not so easy, on after thinking, to account for the general scientific approval of an address which was really an exposure of what the

average cultured Briton regards as the failures of science. Probably the root of the matter is this. With regard to any scientific subjects but their own, scientific men are no other than average cultured Britons. During our conduct of NATURAL SCIENCE this has been thrust upon our notice. It is our endeavour to have subjects so treated that the results of any research shall be intelligible to those who are unfamiliar with the methods of that research. Of course we receive much criticism, but, for the most part, it is not the average cultured Briton who writes to say that a paper on biology, or on geology, is unintelligible. It is the working geologist, apparently, whose standard of the intelligible in biology is the lowest; it is those who are adding to our knowledge of plants and animals that find most difficulty in comprehending what geologists say they are about. Lord Salisbury's address dealt with three or four widely-different branches of science, and we may take it that although his audience included a large number of experts in various branches, yet the verdict of the audience was not the verdict of science, but the verdict of the average cultured Briton. Why, then, is average culture so cheered by the exposition of "the great failures" of science?

AN UNFOUNDED STORY.

IT is reported, in an entirely unveracious chronicle, that after the discovery of America, Columbus returned to Spain and settled down to a life of dignified ease. He became the lion of several seasons, and, as the great discoverer of America, was the favourite of the Court and the darling of the populace. He was invested with every national and civic dignity, and grew a power in the land. When a question of state was being discussed in the council of the king, the king himself would command that the voice of the discoverer be heard. When the populace was seething and troubled, and demagogues shouted against the accredited rulers of the nation, the words of the discoverer of America acted like a charm. But one day, in a moment of expansion, Columbus remarked that there were, no doubt, many undiscovered countries, and that, personally, he had a longing to discover the North Pole. His words were taken up and repeated upon all sides, and dissatisfaction grew with their repetition. Finally, the king himself sent for Columbus, and said:—"Columbus, we honour you greatly and have rewarded you, perhaps lavishly, for your discovery of America; but we must remind you that you have not discovered the North Pole. There is but one America; there are, you tell us, two Poles. We will not bind you harshly to the discovery of either or both. There are two Poles—discover us one of them." And so Columbus, gathering an expedition, set out for the north. When, after years of wandering, he returned, unsuccessfully, he sank into an unhonoured grave, known not as the great discoverer of America, but as the man who failed to find the North Pole.

Lord Salisbury's North Poles of Science.

Two of these undiscovered countries of science (the nature and origin of the elements and the nature of ether) are outside the special scope of Natural Science. But in passing we may observe that, while modern science is as unable to transmute the metals as were the early alchemists, it is the merest dogmatism to say that the boundary of our knowledge in this direction remains where it was many centuries ago. The cataloguing and recording of the elements, the negative results themselves of Stas, the researches of Crookes upon the evolution of the elements, and the grouping of them by Mendeléeff, are all great strides in knowledge. We may be near or far from knowledge of the nature and origin of the elements: it is certainly true that we are nearer the goal by those years of brilliant result and patient research upon which Lord Salisbury justly laid so much stress.

With regard to the nature and origin of life, we should reply to Lord Salisbury in similar fashion. It is quite true that we do not know the exact nature or the origin of life. But we do not regard it as a problem so remote as to rank among the almost impossible quests of science. If to-morrow Professor Bütschli or some other investigator were to demonstrate that the phenomena of life could be called into existence in the laboratory, we should feel that an advance much more momentous in the minds of the populace than in the minds of biologists had been made. For the result of recent research has been to break down the barrier between the mystery of living matter and force and the mystery of inorganic matter and force. Biologists will not accept Lord Salisbury's definition of life as "the action of an unknown force on ordinary matter," as "a mysterious impulse which is able to strike across the ordinary laws of matter, and twist them for a moment from their path." Whatever be the nature of life, it is not a force twisting and thwarting the ordinary laws of matter. Nor is life in any ordinary sense of the term a force at all. If we speak of the chemical or physical work done by living organisms as the results of the force of life, either we use the term as the vaguest metaphor, or we imply that the force of life is only a phase of ordinary physical and chemical forces. For the work done by an organism can be measured in terms of the chemical and physical energies it employs, and the actual growth of an organism, the multiplication of cells, depends upon ponderable transformation of ordinary physical and chemical forces. The results of much recent work on the nature and functions of protoplasm lead many to think that the supposed barrier, between living matter and matter that is not alive, has already been broken down, and that living material is only a peculiarly complicated mixture of known chemical and physical forces. Be that as it may, there is more of the orator than of the thinker in Lord Salisbury's handling of the problem of life. Life, like electricity, or

chemical affinity, or gravity, is not a thing to be explained in a formula. We know the positive and negative conditions of its existence; every month adds to our cataloguing of its phenomena and of its relations to other phenomena. We know nothing but by its qualities, and it is a fair statement to make that science has learned to know as much of the qualities of life as it knows of the qualities of anything. As each step in the slow accumulation of knowledge is made, it will always be open for someone to say, "Ah, my friends, you have discovered America, but how about the North Pole?"

LORD SALISBURY AND NATURAL SELECTION.

DARWINISM and the Doctrine of Descent are now in the unhappy position of popularity. Only those who have some practical acquaintance with the facts involved, with the anatomy and embryology of living animals, and with the history of their fossil remains in the rocks, hesitate to lay down the law for or against what they consider Darwinism. Lord Salisbury's acute mind has avoided the most vulgar error. Since Darwin's solution laid hold of the intelligence of the world and armed it with a new method for the study of organisms, month by month proofs of the doctrine of descent have accumulated, until now no naturalist of position or attainments asserts the immutability of species. But we go beyond Lord Salisbury, and say confidently that there are not six among the well-known biologists of the world who do not hold it as proved that all animals and plants that are alive, or have lived on the earth, are connected with each other by the chain of a common descent. It is upon this achievement that the undying fame of Darwin is based. The dispute about the fact of evolution is over. After the lapse of so many stormy years, the word evolution has earned its right to Lord Salisbury's witty designation "comfortable." It is, as Lord Salisbury rightly said, concerning the method or mechanism of evolution that there is still controversy, and that there may be controversy to the end of time. At the end of the introduction to the first edition of the "Origin of Species," Darwin wrote:—"I am convinced that Natural Selection has been the main but not the exclusive means of modification." At different times after the publication of the "Origin," as may be seen in the different editions of that and other works, Darwin was inclined to attribute sometimes a greater, sometimes a less, important sphere of influence to this factor. At the present time one school, of which Weismann is the leading exponent, believes that Natural Selection, to the exclusion of other factors, is the motive force in evolution. Probably a majority of living naturalists attach to it less importance than did Darwin. But it was because Natural Selection or, "the preservation of favoured races in the struggle for life," furnished an intelligible mechanism for the process of evolution, that Darwin was able to

convince the scientific world of the fact of evolution. Whether or no the final verdict of science be for or against Natural Selection, it will remain the most stupendous example of the value of scientific theory in directing scientific observation.

But Lord Salisbury has regarnished for us two objections to the validity of Natural Selection. The first of these is the question of the exact amount of time during which evolution has proceeded. Now we do not know the duration of time which is expressed by the Tertiary strata. We do know that, relatively to preceding periods, the Tertiary period is but a short episode in the history of the earth. Yet, in this short period, there have come into existence nearly all the species and genera, and many of the families now living. A very small acquaintance with geology shows that, whatever be the case with the method, there has been time for the actual occurrence, for the excellent reason that the occurrence is actual. As for the amount of time being prohibitive of the process of Natural Selection, Lord Salisbury's objections depend upon a view of the mode of operation of Natural Selection which, so far as we know, is peculiar to Lord Salisbury. But it is not necessary to enter into the view of Natural Selection that the President propounded, because his second objection to Natural Selection is, that we cannot demonstrate the process of Natural Selection in detail, and that we cannot even imagine it. This second and final objection is taken by Lord Salisbury from a recent paper of Weismann. To enter into it would be to open up the whole Darwinian controversy. But we may say that we agree with Lord Salisbury that, if Natural Selection implied that for every step "two individuals of opposite sexes, accidentally blessed with the same advantageous variation, had to meet and transmit by inheritance that variation to their successors," then no lapse of time would be sufficient to transform even one species into another. But that, of course, is a burlesque of the supposed process.

We do not agree with Weismann's statement, quoted with approval by Lord Salisbury, that, if Natural Selection be rejected, there is no resource but to fall back on the mediate, or immediate, agency of a principle of design. But we admit fully that if there be no hypothesis to account for the evolution of species but the action of selection upon indefinite variations in each generation, then there is room, and more than room, for the hypothesis of some inherent and directing force of phylogenetic development, which is simply design writ small. On the other hand, every recent publication bearing upon these matters is full of suggestion and argument and fact against the nature and use of variation being such as Lord Salisbury supposes.

Dwarfing by Experiment.

Those of our readers who have paid attention to the recent researches on the manner and causes of variation, to which we have

of late directed their attention, will be interested in a paper that Mr. H. de Varigny has just published in the *Journal de l'Anatomie et de la Physiologie* (1894, pp. 147–188).

In 1874 Professor Semper published the results of some experiments made with specimens of *Limnæa stagnalis*, in order to ascertain the effects of the environment on the growth of the animal, and more especially the causes of dwarfing. By isolating young individuals obtained from the same mass of ova, and placing them in vessels each containing a different amount of water, he found that reduction in the bulk of water, below a certain quantity favourable to full development, was attended by a corresponding reduction in the size of the animals. Since the young snails were fully supplied with food, and placed under similar conditions of temperature and light, and since the state of aëration of the water would be without effect on these air-breathers, the cause of the dwarfing was not very apparent; and Dr. Semper was finally driven to conclude that the reason must be sought in the presence in the water of some substance, not as yet ascertained, which was necessary to the growth of the animal, and the amount of which in the smaller volumes of water was insufficient.

To test these observations more fully, and more especially the Professor's conclusion, Mr. de Varigny undertook a series of experiments. Some of the results arrived at he has embodied in the paper before us. He selected as subjects *Limnæa stagnalis* and a species which he terms *L. auricularis*, but which from his figures is undoubtedly an inflated form of *L. pereger*. Individuals hatched from the same mass of ova were isolated in vessels containing different amounts of water and offering equal or different areas of aëration in the several experiments. The vessels were placed under equal and favourable conditions of temperature and light, and the animals furnished with abundant food. The results went to show, first, as Semper's trials did, that, within limits, the size of the shell increased with the increase of volume of the water the animal was placed in; secondly, that the dimensions of the *Limnæa* increased in proportion to the increase of surface exposed to the air, and that increase was almost more pronounced than in the case of increase of volume of the water alone, and even persisted when the superfices was increased while the volume was diminished; thirdly, other conditions being equal, the size of the shells varied in inverse proportion to the number of individuals inhaling a given volume of water. Semper's conclusion, however, as to dwarfing being due to some unknown substance in the water, was neatly disposed of by experiments in which individual *Limnæ* were penned off from their fellows, and compelled to occupy a small space in the same vessel, while a thorough circulation of the water was effected. The results were just the same as if they had been confined in separate small receptacles.

Seeking, therefore, to explain the cause of this dwarfing in some other way, Mr. de Varigny suggests that it is due to lack of room in

which to move about, a proposition he supports by ingenious arguments, while admitting that further experiments (now actually in progress) are necessary. While looking forward to the publication of these further details, we would venture to commend this paper to the special attention of certain of Mr. de Varigny's countrymen and others in this country, as offering, by means of its figures, a magnificent opportunity for self-advertisement in the creation and naming of limitless varieties and so-called new species.

"A Moving Grove," *Shakespeare.*

In our June number (vol. iv., p. 406) we had something to say about the way in which animals were protected from discovery by their resemblance in colour or form to surrounding objects. Now a man, if he wishes to escape observation in this manner, does not change his shape or the colour of his skin, but clothes himself in some appropriate substance, or, like the soldiers of Macduff when they advanced on Dunsinane, hides himself beneath a covering of natural objects. Just in the same way, though possibly without the same conscious intent, certain of the very humble dwellers in our modern seas take to themselves a cloak fashioned from the common objects of the sea-floor. Thus, a certain coiled shell has received its name, *Xenophora*, "stranger-bearer," from this very habit; for the molluscs of this genus attach to their shells, which are usually very thin, such things as stones, shells and pieces of coral, which are found at the bottom of the sea in which they dwell. Three such specimens have recently been placed in the central hall of the Natural History branch of the British Museum, one of them, *Xenophora pallidula*, from Japan, and the others, *X. conchyliophora*, from Bermuda. One of them is covered with a number of other shells, and another with stones and sand; and it is interesting to see how large these foreign bodies are in proportion to the fragile shell itself, and how completely they hide it. Close by them there is exhibited a crab, *Maia squinado*, which came from Plymouth, and is entirely covered by fragments of seaweed, sea-mats and sea-firs. These organisms are not merely growing on the shell of the crab, but are attached to it by the hooked hairs with which the exposed surface of its body and limbs is clothed.

These cases are interesting enough, but a further and more wonderful development of the same method of protection is found in certain sea-urchins that inhabit the harbour of Kingston in Jamaica. Their names are *Toxopneustes variegatus* and *Hipponoë esculenta*, and unlike many other sea-urchins or sea-eggs, as they are called in Jamaica, these species do not stay at home in holes or crevices, but wander about the smooth, hard bottom, in very shallow water, where many sea-weeds are growing. But like other sea-urchins, these animals are provided with little suckers or tube-feet, and while they use some of these to drag themselves along, they use the suckers on their backs

to carry about bits of sea-weed and shell and small pebbles. By this means they very successfully conceal themselves from the naturalist and no doubt from other enemies; but the edible *Hipponoë* is, unfortunately for itself, less an adept at the practice than is the species of *Toxopneustes*.

NEMATODES IN THE BLOOD OF MAN.

THE English race, among the many interesting things which they bring back from the tropics, occasionally unwillingly bring back for the consideration of English medical men parasites of the tropics. In the new issue of the *Transactions* of the Norfolk and Norwich Field Naturalists' Society Mr. H. D. Geldard describes an interesting case of *Filaria sanguinis hominis* in the blood of a patient who resided many years in India. The specimens abounded in the blood of this patient, and were that larval phase of *Filaria bancrofti* which is called *Filaria nocturna*, as it appears in the blood of man only from sunset to sunrise. These young embryos are taken from the blood by the female mosquito, which also is a nocturnal animal. A few days after its meal the mosquito retires to water to deposit her eggs, after which she dies, and her dead body drops into the pond or stream. Within her body the embryos which had got into the alimentary canal from the blood of man have been rapidly maturing. They issue from her dead body by means of a boring apparatus developed at their anterior end. They swim freely through the water, and apparently ultimately perish unless they are swallowed in the water by man. If this happens they bore their way from the stomach to the lymphatic vessels, where they become sexually mature. Each female pours out a multitude of small embryos, each within a delicate egg-shell. These find their way into the blood-vessels and lie concealed, except at night, when the mosquito is active, and so gives the opportunity for this cycle of development to begin again. The consequences of their presence in man may be trifling, but by choking the lymphatics they may cause some most unpleasant and dangerous diseases.

HOP-DISEASE IN KENT.

WE learn from Professor J. Percival, of the South-Eastern Agricultural College, Wye, Kent, that he is at present engaged investigating the disease which causes hops to become what is known as "nettle-headed." The disease is met with in many districts in the county, and has rapidly increased during the last few years—in some cases leading to a complete destruction of considerable areas of hop gardens. The early delicate varieties are most attacked, and especially those which have been growing for a considerable time. It is rarely until five years after planting that the disease is seen, and usually not until a much later period.

In a typically affected plant the leaves are smaller than is generally the case, and are transformed in such a manner as to become strikingly like those of the stinging-nettle. The edges of the leaves turn upwards; the fibro-vascular bundles in them are increased slightly in thickness, and stand out well from the under surface of the leaf.

Closer examination shows destruction of the soft tissue in immediate contact with the fibro-vascular bundles; the parts become thin and yellow, and much resemble the thin spots produced by the action of sulphur dioxide gas on the leaf.

The internodes of the plant are short and the main stem and branches taper more rapidly than in healthy plants. In practically all cases the bine or stem, after climbing a short distance up the pole, loses it power of twining and topples over; when not tied up the whole stem becomes slack, slides down its support, and the plant lies in a heap on the ground.

So far the experiments, which are being carried out on the Bettring estate of Messrs. E. A. White & Co., Paddock Wood, point to the circulation within the plant of a poison which brings about curling of the leaf, stoppage of growth of the fibro-vascular tissue, and loss of turgor of the cells in the leaf near the ribs, with resulting yellow patches there.

The leaves on the main stem nearest the ground are affected first, and then follow those at the nodes above in regular succession. The branches in the axils of these show the same regular sequence of diseased leaves, the lower branches being affected first. The cause is undoubtedly connected with the root, and Professor Percival finds in the cortex of many diseased specimens, close up to the bast, considerable numbers of the nematode *Tylenchus devastatrix*, Kühn, usually known as an eel-worm.

Before any completely satisfactory cause can be established or remedy suggested, much work is necessary. At a later date we hope to be able to give an extensive communication from Professor Percival dealing with this interesting and important investigation.

The Indian Geological Survey.

The usefulness of the Geological Survey of India appears to be fully recognised by the practical men of the Dependency. Thus, we read in the *Records*, vol. xxvii., p. 65, that, in his inspection of the Bengal collieries, Mr. Grundy, the Inspector of Mines, has "experienced very earnest co-operation on the part of the agents and managers of the mines." On the other hand the Survey is not blind to the advantages it may itself gain by friendly co-operation with the practical men. The maps of many large coal-fields that were made some twenty and thirty years ago, on the largest scale available at that time, are now found to be insufficient. It is suggested therefore

that the Bengal Colliery managers should form a committee to prepare a revised and enlarged map of the Ranigunj coal-field. "The managers would appear to have quite sufficient data before them in the way of borings and shaft sections, which, combined with their local knowledge, should enable them to contribute materially to the production of a lasting standard work of this kind. An initiatory step might be made by supplying managers with copies of the latest revenue and other maps of their districts, on which each gentleman could enter all the data at his disposal according to some settled scheme of geological and mining delineation. The Geological Survey Staff is all too limited for such an undertaking, but by judicious co-operation with a committee of the kind indicated, a great deal might be done to effect the desired end." A concrete instance of the assistance that may be so rendered is already to hand in an excellent map of the Giridh coal-field by Dr. Walter Saise, manager of the East Indian Railway Collieries, which is to be presented to the Survey for publication. It is distinctly cheering to find that the Indian Geological Survey does not snub the work of private observers in the way that some other Government Departments are accused of doing.

Tertiary Rivers.

The little island of Eigg in the Inner Hebrides has long had a fascination for the geologist. Readers of Hugh Miller's charming book "The Cruise of the Betsey" will remember his description of the Scuir of Eigg, that large wall of curious pitchstone-porphyry that surmounts the southern end of the island, running across it from E. to W. This wall appears to be the remains of a once far larger mass of lava, which flowed down from some adjacent volcano and filled a river valley. The river deposits of this valley, with remains of a pine, are preserved beneath the Scuir. At the meeting of the British Association, Sir Archibald Geikie drew attention to a little rock called Hysgeir, which lies in the sea some eighteen miles west of Eigg. This rock consists of just the same peculiar porphyry as the Scuir, but lies at a much lower level. It therefore seems to prove a further extension of the river-valley, and a descent of it about 35 feet in a mile. Some other islets north of Hysgeir exhibit records of a still older river. In what must have been the valley of this river there are found a succession of coarse river gravels alternating with bedded basalts. The lowest of these gravels contains remarkably large blocks, some as much as six feet in length. Some of the gravels or conglomerates pass into true volcanic agglomerates; while in other places there are layers of fine tuff, volcanic sandstone, or shales with remains of land-plants.

The sequence of events which these various deposits indicates was described by Sir Archibald as follows. "During the outpouring

of the lavas of the great basaltic plateaux of the Inner Hebrides, a river flowed across the volcanic plain from the Western Highlands, whence it carried large quantities of shingle. By successive violent floods, these materials, together with the detritus of the lava-fields, were strewn irregularly far and wide beyond the immediate channel of the river. In the pools thus left behind, fine volcanic silt gathered and entombed leaves and stems of the surrounding terrestrial vegetation. But volcanic activity still continued, and though ashes, slags and pumice were swept down, new eruptions took place by which masses of rock, sometimes nine feet in diameter, were thrown out to a distance of a mile or more, and fresh streams of lava were poured out, completely burying the previous accumulations. Renewed river-floods of gradually-lessening severity spread fine detritus over the cooled sheets of basalt, and again these later fluviatile deposits were entombed between fresh outbursts of lava. Perhaps no more striking evidence can be elsewhere obtained of the condition of the land surface over which, from many scattered vents, the materials of the volcanic plateaux of the Inner Hebrides were slowly piled up." It is interesting to find that in these far-distant Tertiary times the slope of the drainage areas of the Western Highlands was the same as in our own day.

Landscape Marble or Cotham Stone.

The curious deposit known as Cotham Stone has exercised the ingenuity of many to find a cause for its formation. As early as 1754, Edward Owen published a work entitled "Observations on the Earths, Rocks, Stones and Minerals for some miles about Bristol, and on the Nature of the Hot Well and the Virtues of the Waters," and in this book he suggests that the peculiar arborescent structures of the Cotham Stone were produced by the escape of imprisoned air, which permitted the blacker portions of the mud to follow the bubbles, and so get dispersed in tree-like patches. Others have thought that gaseous emanations from the *Avicula contorta*-shales had produced the peculiar markings, and others again suggested that mineral infiltrations would account for them.

Mr. H. B. Woodward in 1893 (Mem. Geol. Surv. The Jurassic Rocks, vol. iii., p. 31) gave it as his opinion that the "arborescent markings were produced during the consolidation of the stone, and more particularly by the shrinking of its upper portions. In this way, and while the mud was still in a more or less pasty condition, one or more of the dark films in the banded mass were disarranged and dispersed in arborescent form in the slowly-setting rock." He suggested also that pauses in deposition and exposure to the sun's rays might have accounted for some of the structure, and concluded that markings were due mainly to mechanical forces, though there were evidences of chemical change.

Mr. Beeby Thompson, in the last number of the *Quarterly Journal of the Geological Society* (August, 1894), has examined all previous writings on the subject, investigated the matter, and concludes that the peculiar characteristics are due to interbedded layers of vegetable matter, which continued to decompose and evolve carbonic acid gas and marsh gas after deposition; and that where a layer of extra thickness occurred, the decomposition continued whilst a thickness of several inches of new sediment was laid down, with the result that arborescent markings were produced along the lines taken by the escaping bubbles. Mr. Thompson, in this interesting paper, gives his reasons for these conclusions, and records his experiments to reproduce artificially the characteristics of landscape marble.

The Systematic Position of Trilobites.

For many years the position of the Trilobites in the scale of organised beings has been doubtful. One school has insisted that they should be referred to the Crustacea, and another to the Arachnida. Mr. H. M. Bernard has just published, in the *Quarterly Journal of the Geological Society* (Aug., 1894), an important zoological paper in which he discusses at length the question, and refers these ancient life-forms definitely to the Crustacea, the original position assigned to them by Linnæus and by Burmeister. We are unable to give the details of Bernard's paper, but quote his own words as given in his "summary":—

"It is now possible . . . . to fix with great probability the zoological position of the Trilobites. The bending round ventrally of the first segment, the great labrum with antennæ attached at its sides, the 'wandering' of the eyes, the pores (pointing to the probable presence of water-sacs), the head with a varying and progressively increasing number of segments, the dorsal organ, the rudimentary character of the posterior segments, and the gradual diminution in size, with the essentially lobate or phyllopodan type of the limbs, all serve to connect the Trilobites with *Apus*.

"This relationship cannot, however, be considered as direct. *Apus*, on account of its richer segmentation, the absence of pleuræ on the trunk-segments, and its more membranous parapodia-like limbs, must be assumed to lie in the direct line upwards from the original annelidan ancestor towards the modern crustacea. The Trilobites then must have branched off laterally from this line either once or more than once, in times anterior to the primitive *Apus*, as forms specialised for creeping under the protection of a hard imbricated carapace. This carapace was obtained by the repetition, on the trunk-segments, of the head-shield which, as we have already seen, almost certainly existed as a structure *sui generis* in earlier forms, and, somewhat modified, has been retained as such in the early crustacea proper ('Aspidophora').

"It seems to me, therefore, that the Trilobites, studied in the light of new discoveries, especially of those which we owe to American investigators, yield the most interesting and important evidence as to

the origin of the crustacea. Stripped of their pleuræ and of the expansion of the head-shield, we have, in the early Trilobites (*e.g.*, *Olenellus*), long segmented animals tapering at the posterior end. The first segment is bent round ventrally, so that the large labrum points backwards. The appendages of the first segment appear to have functioned as sensory organs and to have pointed downwards, being inserted at the sides of the labrum. The following segments were provided with membranous lobate appendages carrying, on their dorsal edges, gills and sensory cirri, and distally specialised into locomotory organs. The alimentary canal ran through the whole length of the body, bending round anteriorly to open through the mouth.

"The Trilobites may thus be described as *fixed specialised stages in the evolution of the crustacea from an annelidan ancestor, which bent its mouth round ventrally so as to use its parapodia as jaws.*"

To us it has seemed in the highest degree improbable that crustacea could have come from a specialised annelidan, but it will be interesting to see what the "arachnoid" theory-holders have to say in reply to this paper, which appears to be carefully and thoughtfully worked out. It is fully illustrated, and marks a distinct revival of palæozoological interest in the Geological Society which we are pleased to see. We note, however, that of those four who entered into the discussion of the paper, three were zoologists, geologists as a rule caring for little else than stratigraphy or petrology.

Species-Making and Species-Taking.

In one branch of biology there still rages a battle about species. Those who are concerned with the collection, naming, and description of fossil and living species are troubled constantly by the stupid and unprincipled action of species-mongers of the baser sort.

Opposed though we are to too great extension of professionalism or officialdom in science, we have sometimes wished that there could be something in the nature of a diploma without which no one should be permitted to practice, *i.e.*, to publish, or that there were some means of disabling an incompetent or dishonest writer, such as the Bar exercises when it strikes a man off the rolls. Even now there is a case that, if the facts be correctly stated, certainly calls for some such drastic treatment. A writer named Victor Lopez Seoane has recently been distributing a couple of pamphlets, entitled "Aves nuevas de Galicia" and "Revision del Catalogo de las Aves de Andalucia," both of which bear the imprint "La Coruña, 1870." There is, however, clear internal evidence that the date of one of these is incorrect, as it refers to the British Museum Catalogue of Birds, the first volume of which did not appear till 1874. Moreover, it is stated by Mr. C. D. Sherborn, who discusses the question in the August number of the *Annals and Magazine of Natural History*, that when these pamphlets reached him "the ink in which they were printed was apparently fresh and easily smeared." Whether or no it is designed for the purpose, this pre-dating gains priority for some

of Seoane's own names over names proposed by Howard Saunders and Reichenow. The sooner such writings are ruled out of court, the better it will be for systematic zoology.

Another case of what seems illegitimate "Species-making and Species-taking," is illustrated by a little polemic now proceeding between two well-known palæontologists, Professor Ralph Tate, of Adelaide, and Dr. Bittner, of the Royal and Imperial Geological Institute of Vienna. Every now and again some "lumping palæontologist" figures two specimens to represent the same species; then, to his annoyance, one of the "splitting" students of that science promptly makes a new species out of the second figure. We have all heard of cases in which some over-zealous member of the *nouvelle école* of conchology has made a second species out of a different view of the same specimen. Dr. Bittner has not been so unscientific as this. Some time ago Professor Tate sent to Vienna a collection of specimens of the Australian Lower Tertiary Echinoidea. As this fauna has not yet been fully worked out, Professor Tate asserts that he was very careful to send only well-known species, and to select what he regarded as most typical forms. Nevertheless, so different are Dr. Bittner's and Professor Tate's views of specific differences, that the former subsequently issued a paper ("Ueber Echiniden des Tertiärs von Australien." *Sitzber. Ak. Wiss. Wien.*, Bd. ci., Abth. i., pp. 331–371, pls. i.–iv.) in which he founded seven new species and one new variety on this material. That he also established two new genera for two species which had been previously described was not surprising. To this Professor Tate has replied in some "Critical Remarks on A. Bittner's 'Echiniden von Australien'" (*Trans. Roy. Soc. S. Australia*, vol. xv., pp. 190–194), in which he refuses to accept any one of Bittner's new species, and maintains that these are all founded on specimens as typical of the old species as any that could be selected. He ascribes Dr. Bittner's error to his ignorance of the specimens as they occur in the rocks, a cause which is undoubtedly efficient in many other instances of splitting palæontology.

I.

Rev. George Henslow on Natural Selection.

IN NATURAL SCIENCE of July Mr. Henslow makes some statements with regard to variation and Natural Selection which call for critical remark. He says that, though cultivated plants vary *indefinitely*, and therefore require selection to produce definite modifications, this is not the case in Nature—"Variation in Nature is always in strict adaptation to the direct action of the environment; in other words, *natural variation is always definite*." This statement seems to me so extraordinary, and so opposed to well-known facts, that I can only impute it to the use of the terms "vary" and "variation" in two very distinct senses; first, as meaning those individual variations which occur abundantly both in nature and under cultivation; and, secondly, as meaning those particular variations which alone survive under nature and produce a "variety" or a "species." In this latter sense, of course, "natural variation" is definite; but so, in the same sense, are the variations of cultivated plants. From unstable and indefinite "variations" man and nature alike produce definite "varieties." As one out of the innumerable examples of indefinite variation which might be named are the fifteen different *modes* of variation observed by Alph. de Candolle on a single oak tree, while in a great number of common species an equal amount of variability may be observed both in wild and cultivated individuals; and all these variations are *indefinite*, in the sense that they do not usually occur in one direction only, from the typical form. A few examples of such variations have been given in my "Darwinism," pp. 76–80. I cannot, therefore, understand either the meaning or the value of the statement—"*natural variation is always definite*."

It is not quite clear whether Mr. Henslow admits the agency of Natural Selection at all. He says: "I would ask what *facts* are producible to *prove* that Natural Selection acts at all on the maintenance, if not the origin, of any floral and, indeed, other structures?" It is, of course, admitted that *direct* proof of the action of Natural Selection is at present wanting; but the indirect proofs have been so cogent as to overcome the most violent prejudice and opposition, and to convert a large majority of naturalists to a belief in its agency. It is, therefore, rather late in the day to deny its existence without

adducing some adequate and proved substitute. What Mr. Henslow does put in its place is the reaction of vegetable tissues to the environment, resulting in adaptation; and in the special case of flowers he imputes all the variety of form and endless modifications of structure to "the responsive action of the protoplasm, in consequence of the irritations set up by the weights, pressures, thrusts, tensions, etc., of the insect visitors."[1] Now the very first essential to this theory is to prove that modifications produced by such irritations are hereditary. Here, if anywhere, we want facts. Yet in the very interesting volume to which Mr. Henslow refers us, crowded as it is with facts and observations, I can find only two or three slight references to this most vital point. At page 147 he quotes Darwin as saying that the excellence of our milking cows and goats may be attributed partly to selection and "*partly to the inherited effect of the increased action, through man's art, of the secreting glands*," and adds, "This fact" is strictly analogous to what takes place in the vegetable kingdom! Here we have a mere opinion of Darwin's, nowhere supported by direct observation or experiment, and now seriously challenged by a large body of naturalists, set forth as "a fact." Again, at page 157, the case of the various "ant-plants" of the eastern tropics is referred to, and it is stated that Dr. Beccari explains the curious hollow stem in which the ants dwell as partly due to the irritation of the ants inducing hypertrophy of the vegetable tissue, which "then becomes hereditary"; and Mr. Henslow concludes that there is abundant evidence to prove that many organs of a plant, if subjected to irritation, can become materially altered and develop new processes, and, "secondly, that these altered states, if the irritation be persisted in, may become hereditary." Here again are only opinions without a particle of proof; and I can find nothing more to the point in the whole volume. The case of galls is very briefly referred to at p. 144, and their non-heredity is passed by with the remark that the predisposition to produce them *may* be greater now than formerly, and that the galls themselves *may* be larger than they were at first. But surely if the effects of insect irritation are anywhere hereditary it would be here. An oak tree which lives several hundred years is subject to this irritation in greater or less degree almost every year, and the irritation itself is not momentary and intermittent, as in the case of insects visiting flowers, but is kept up by the presence of the egg and growing larva during a considerable portion of the period of active vegetable growth, and this has been going on for thousands, probably millions, of years. Yet neither do oaks nor any other plants produce galls spontaneously, as they certainly should do if the results of irritations are in any general sense hereditary. This seems to me to be a really crucial experiment continually repeated by nature.

I may here remark that Mr. Henslow's theory utterly breaks down owing to the want of any conceivable connection between

[1] "The Origin of Floral Structures," p. 340.

insect irritation and most of the innumerable adaptations of the parts of flowers to attract insects and secure cross-fertilisation. Such are the sticky glands, the elastic filaments, the springs and traps, and the accurately timed motions of the pollinia in orchids; the innumerable complexities in papilionaceous flowers; the large coloured tracts in Bougainvillea, Poinsettia, and many others; the flowers with tightly-closed lips, as Linaria, Antirrhinum, Melampyrum, etc.; the enlarged rays of Compositæ, Umbelliferæ, and Caprifoliaceæ; the general massing of small flowers into heads, umbels, corymbs, or dense racemes, so as to become conspicuous, and many other characters. To these may be added the negative evidence of the numerous genera and orders of regular flowers, such as Campanula, Rosaceæ, Gentianaceæ, and many others, which, though thoroughly adapted for insect fertilisation, and whose lower petals have therefore been always subject to irritations, have never developed irregular flowers. In all these cases variation with Natural Selection will account for the phenomena, while insect irritations, even if we admit heredity, will not do so. From whatever point of view we approach the question, the attempt to explain floral structure and colour without the aid of Natural Selection is a hopeless failure.

In the *Journal of the Linnean Society* ("Botany," no. 208, July 10) there has just appeared an elaborate paper by Mr. Henslow on "The Origin of Plant-Structures by Self-Adaptation to the Environment, exemplified by Desert or Xerophilous Plants," in which the author still further develops his view as to the influence of the direct action of the environment unaided by selection. The only portion of this paper on which I propose to remark is that dealing with the origin of spines and prickles, on which I have already had occasion to write in my book on "Darwinism," when combating Professor Geddes' views on the same subject. Mr. Henslow imputes the spines and prickles of so many plants inhabiting dry countries to the direct influence of the conditions under which they live. This, he thinks, is proved by some of these plants losing their spines when grown under other conditions; he adduces numerous examples of the abundance of spiny plants in such countries as Nubia, Abyssinia, and the Kalahari Desert; and he again and again reiterates the statement that these characters are "simply the inevitable results of the action of environment."

Now if these statements comprised all the facts, that is, if in all dry countries spiny plants abounded, while in all moist or fertile districts they were absent or very rare, the explanation given of their origin would have some plausibility. But there is no such general coincidence of aridity of soil or atmosphere with abundance of spiny plants, as very little enquiry will show. Mr. Henslow points out several other plant-characteristics which indicate, and, as he thinks, are directly caused by, aridity. Such are very small, coriaceous, or rolled up leaves, or their complete absence; a hairy or woolly covering

to the whole plant; succulent foliage; special protection of the buds; enormous development of roots; abundance of bulbs and tubers; together with thickness of bark and various protective coatings to stems and leaves. Now many of these peculiarities are present in the flora of the Brazilian Campos—as well described in the memoir of Eug. Warming on Lagoa Santa—which is referred to by Mr. Henslow as corresponding in many respects with that of other arid regions. Yet the author of this memoir expressly states that "spiny plants are very rare" (p. 463). Again, the plants of the Galapagos present similar indications of aridity—shrubs with minute and almost invisible leaves, for example—yet, except the cacti, which may be of American origin, none of the endemic species are spiny. So, also, the rich Sandwich Island flora contains hardly a single endemic spiny plant; and I am informed by the Rev. R. P. Murray, who is well acquainted with the botany of the Canaries, that spiny plants are exceedingly rare in those islands, though much of the surface, owing to the porous volcanic rock and the long periods of drought, presents the conditions which elsewhere are said to produce spines.

Now without denying that—other conditions being equal—aridity may favour and moisture may check the growth of spines, there is another and altogether different set of conditions which seem more directly connected with their abundance or rarity. This is, the presence or absence of herbivorous mammals, against whose ravages spines are a protection. The most destructive of these animals are camels, goats, and antelopes, and it is where these are indigenous—in Arabia, North-east and South Africa, and Central Asia, that thorny shrubs and trees are especially abundant. Again, few countries have more spiny plants than Chili, where the camel-like vicuguas and alpacas, as well as large rodents, are very destructive. But the country is not especially arid, and the remarkable Puyas, whose leaves are armed with excessively sharp recurved spines, inhabit the subalpine regions where rain and mist prevail. In our own moist islands we have a full proportion of prickly plants, and the same may be said of North America, where the *Gleditschia* or Honey Locust has the young branches, and in old trees the trunk, armed with groups of very strong and sharp spines. So also in Japan, notwithstanding its moist insular climate, we have an *Olea* and an *Osmanthus* with holly-like prickly leaves; while the prickly *Berberis Darwinii* is found in the damp atmosphere of the Straits of Magellan.

Equally opposed to the theory of aridity as the efficient cause of spines is their abundance on palms growing in the hottest and moistest regions of the globe. In many Amazonian species the stem is thickly set with long and very sharp spines pointing downwards, and thus forming a complete protection against monkeys and other arboreal fruit-eating mammals. Many species of *Bactris* and *Astrocaryum* are thus armed, as is also the beautiful *Guilielma speciosa*, the Peach palm, whose fruit is large and edible. It is a suggestive

circumstance that, with the exception of palms, few large trees are spiny, and when they are so, as in the case of the *Gleditschia*, the spines are most abundant on the trunk and on the younger branches. In the same way, our holly, when it grows to a large size, usually has the leaves towards the top spineless: the wild pear also is spiny below but unarmed above. The climbing palms, on the other hand, are armed to the very top, but in this case the spines assist climbing.

The anomaly of the flora of the Brazilian Campos having most of the true xerophilous characteristics, yet being almost wholly without spiny forms, is quite in harmony with the fact of the great poverty of this region in mammals destructive of woody vegetation. There are really none but a few deer and cavies, which are mostly inhabitants of the more wooded valleys, and which are kept from undue multiplication by the considerable number of species of Felidæ and Canidæ in the same area.

We are, therefore, led to conclude that the apparent direct dependence of an unusually spinescent vegetation on arid conditions of soil or climate is to a great extent deceptive. Such conditions are inimical to the growth of dense forest, and it is a well-known fact that the larger mammalia abound most in partially wooded or open country. Many of these animals are exceedingly destructive to shrubby or aborescent vegetation, especially in districts which are subject to occasional droughts; and it is in such areas that so many of these plants have acquired the protective armature of spines or prickles, while others not so protected have sooner or later succumbed, thus leading to a preponderance of the former. But the numerous instances in which considerable areas and extensive floras are found to have hardly any spinous plants, as compared with other areas in which the soil and climate are generally similar and where such plants abound—the only important difference being the absence or presence of destructive herbivorous or frugivorous mammals—show us clearly that it is the latter rather than the former condition which is the real starting point and efficient cause for the development of spines, while the mode of their production has been through spontaneous variation and Natural Selection.[2]

A few remarks may now be added on the general question of adaptation in the vegetable kingdom. Reference has already been made to the numerous cases in which the special adaptations of flowers to insect-fertilisation can by no stretch of imagination be imputed to the direct action of insects, and the same thing is equally clear in many other directions. The whole group of insectivorous plants, for instance, exhibit strange and complex adaptations which have no

[2] Professor A. Kerner gives an admirable account of the various forms of spiny and prickly plants, which are exceedingly numerous in the Mediterranean district, and he adds: "In northern regions not exposed to summer drought, where grazing animals find in summer enough green fodder, this form of plant is almost entirely absent." ("The Natural History of Plants," English Translation, vol. i., p. 445.)

direct relation to the mere fact of insects crawling over them or settling upon them. So also are those varied adaptations by which, as Kerner has shown, injurious insects are prevented from reaching the flowers.

Even more unintelligible on this theory are modifications of fruits and seeds, by which some attract birds or mammals to eat them, while others are guarded against being eaten; some seeds have beautiful wings or plumes for wind dispersal, others have hooks or sticky hairs which cling to wool or feathers, while others again are scattered abroad by the sudden elastic bursting of the capsules. Take the comparatively simple case of nuts. Did they acquire their hard covering and brown protective tints and detachment from the tree as soon as ripe by the direct agency of birds, or monkeys, or squirrels? Of course, the question is absurd, since those eaten by these creatures could not transmit their special qualities; but those that, by the possession of any of these qualities, escaped being eaten, would transmit those qualities to the next generation.[3]

Any conceivable direct action of the environment can therefore have produced only a very small portion of the modifications and adaptations that actually exist. In by far the larger number of cases no such explanation is possible, and no other adequate explanation has been suggested except variation and Natural Selection. It is, of course, admitted that the action of the environment does produce definite changes in all organisms, more especially in plants, but there is no evidence that such changes are transmitted to the offspring of the individuals in which they have been produced.

On the other hand, there is direct evidence that many such changes are not transmitted, an example of which is the *Arabis anachoretica* with remarkable tissue-papery leaves, due to its growth in hollows of the rock, where neither sun nor rain reach it. Seeds of this plant when cultivated at Kew produced the common *Arabis alpina*. The same thing occurs with many plants, as every cultivator knows; but other forms with no greater peculiarities externally preserve their characters under cultivation, though exposed to the most varied conditions. As we thus know that *some* variations directly due to the environment are *not* transmitted, and also know that an immense number of spontaneous or congenital variations *are* transmitted, since by taking advantage of this fact almost all the improvement in our domestic animals and cultivated plants has been effected; and yet further, that no case has been found in which such spontaneous variations are wholly intransmissible,—the logical conclusion is that the two kinds of variation are distinct in their nature. This view of the subject is adopted by those botanists who are now endeavouring to determine the true nature of the

[3] Other cases of the want of relation between adaptations and their supposed cause are given in my article, "Are Individually Acquired Characters Inherited?" in the *Fortnightly Review* of May, 1893, pp. 664-9.

numerous alleged species, sub-species, and varieties of our native plants. They test the fixity of the characters which distinguish each form by cultivation. If these characters remain unchanged, and are transmitted by seed, the form is a permanent one and deserves to be recorded as a species or sub-species; but if, as frequently occurs with forms which appear quite as distinct as those which are stable, the plant reverts on cultivation to some other form, it is evidently a modification due to some local conditions of the environment, and should be treated differently. Mr. Beeby has proposed to call the former "intrinsic," the latter "extrinsic" varieties, terms corresponding to Weismann's "germ variation" and "somatic variation," and these can in many cases only be distinguished from each other by the test of cultivation under different conditions. On this point Mr. Beeby remarks:—"The most transient states of plants due to the direct action of their environment are often far more distinct in appearance from their normal forms than are some varieties from their types; but the first-named return at once to their normal state on being removed from their special surroundings, while the latter remain permanently distinct from their types even when grown under circumstances most disadvantageous to the continuation of the particular variation. That these two kinds of variation exist in plants is certain; and the separation of them seems to be the very basis on which all investigations of the Phanerogamia must be made, if it is hoped that this branch of botany is to throw any further light on Evolution."[4]

In conclusion, I submit that the whole body of facts in relation to the direct action of the environment indicates that modifications thus produced in the individual are not transmitted to the offspring; and that until it is demonstrated by experiment that they are so transmitted, theories of plant modification founded on that assumption are altogether worthless.

ALFRED R. WALLACE.

[4] "On the Flora of Shetland." *Annals of Scottish Natural History*. January, 1892, p. 52.

II.

Hertwig's "Preformation or New Formation." [1]

Part II.—Criticism of Weismann's Theory of the Germ-plasm and Doctrine of Determinants.

ALTHOUGH Weismann has carried out his theory into the greatest detail, it is not with details so much as with the fundamental principles of it that Hertwig disagrees. It is with these principles, as they concern what is known about cells, that the chief difficulties seem to him to occur. No doubt, as Naegeli has said, there are many units of different orders above and below the cell, but the observation of the last thirty years has made it more and more certain that the cell is the most striking morphological and physiological unit of the body. Every theory of heredity must be tested by comparing it with facts observed about the cell. The cell, consisting of protoplasm and a nucleus, is an elementary organism, which, by itself or in combination with others, forms the basis of all organisms. It has an inconceivably complex structure, which, in its minuter parts, eludes our observation, and it is composed of many physical substances. Of these, albuminates, fats, carbohydrates, water, salts free or in solution, serve as material for the growth of the cell. Others form the living cell-body, in the narrow sense: by growth and division they can multiply and form the elementary unit of which the cell is a higher combination. In this category of intra-cellular units would be placed, if they existed, such theoretical structures as the gemmules of Darwin, the physiological units of Herbert Spencer, the bioplasts of Altmann, the pangenes of De Vries, the plasomes of Wiesner, the idioblasts of O. Hertwig, and the biophores of Weismann. The cell of each species has its specific organisation of a simpler or more complex structure, and contains a proportional number and variety of organised lower units. An unfailing organ of every cell is the nucleus, which is composed of elementary living units, the idioblasts, which differ chemically, morphologically, and physiologically from the units of the protoplasm, but may, perhaps, be able to turn into them. As a matter of observed fact, the

[1] Zeit- und Streitfragen der Biologie. By Professor Dr. Oscar Hertwig. Pamphlet I. Präeformation oder Epigenese? Grundzüge einer Entwicklungstheorie der Organismen. Pp. 144, with 4 illustrations in the text. Jena: Gustav Fischer, 1894. Price 3 marks.

nucleus is the bearer of the hereditary plasm, and may be inferred to be more stable than protoplasm and less subject to outer influences. It is the inherited plasm of the nucleus that stamps the specific character of the organism. The capacity of the cell to feed, grow, and divide is the mechanism by which the continuity of life is secured.

The cell plays a leading part in Weismann's theory. His germ-plasm is made up of primary units, the biophores, which are built up into determinants, a determinant existing for each independently varying cell or group of cells in the adult organism.

The arrangement of the biophores in the determinants, and of these in the ids or higher units, follow an elaborate historical architecture. This complicated germ-plasm lies in the nucleus, and nuclear division is the mechanism by which it passes from cell to cell. An individual life-history begins with a fertilised egg-cell containing paternal and maternal germ-plasm, which have come respectively in the nucleus of the ovum and in the head of the spermatozoon. In the actual course of ontogeny two things happen. By a series of nuclear divisions some of the germ-plasm is saved in an unaltered form, and ultimately gives rise to the sexual cells of the new individual. The structure of the rest of the germ-plasm in the course of another series of nuclear divisions gradually breaks down in such an orderly fashion, that the various determinants are marshalled to their proper places until ultimately the tissue cells of the various parts of the embryo and the adult, instead of containing a complicated plasm, contain only the determinants of their own order. Thus nerve-cells contain only the determinants of nerve-cells, muscle-cells of muscle-cells, and so forth. In certain special cases, which Weismann believes to be secondary adaptations, in addition to the specific determinants, cells may contain some other latent determinants, to provide for the regeneration of lost parts and so forth. But, with these exceptions, there is in every organism an impassable separation between the unaltered germ-plasm, which has been preserved to pass into the sex-cells, and the specialised simplified nuclear plasm of the various independently varying parts of the organism. Moreover, inasmuch as the unaltered germ-plasm of the sexual cells is continuous from generation to generation, while the part of it which breaks up into the nuclear matter of the various tissue constituents perishes with these at the death of the organism, Weismann distinguishes the one as the immortal germ-plasm, the other as the mortal somatic plasm.

Comparing this theory with actual knowledge of cells, Hertwig points out that it implies two quite different kinds of nuclear division. That kind of nuclear division by which the unaltered germ-plasm is handed on from cell to cell until it reaches the new sex-cells may be called "heirs-equal" division (erbgleiche Theilung), for in it the nuclear matter is distributed equally and unaltered between the two daughter-cells. That kind of nuclear division which occurs

while the various determinants are being marshalled to their places may be called "heirs-unequal" division (erbungleiche Theilung), for in each stage of it the daughter-cells receive different sets of determinants. Thus, if, as Weismann supposes, the first cleavage plane determines the longitudinal axis of the future body, the daughter-cell on the right would receive all the determinants for the right side of the body, the daughter-cell on the left all the determinants for the left side. This process of heirs-unequal division would be the characteristic method of nuclear division in all ontogenies, heirs-equal division occurring only in the few cases necessary to separate off the sexual cells. Later on in development, when the determinants have been completely sorted out, heirs-equal division would re-establish itself, for a determinate or independently variable part of the body generally would consist of a large number of cells resulting from the simple numerical multiplication of the cell or group of cells which, by heirs-unequal division, had received the complete set of determinants for the tissue in question.

Now Hertwig strikes at the root of all this process, and therefore at the root of the whole Weismannic theory of heredity, by saying that heirs-unequal division does not occur. Against it he brings up five groups of facts.

Groups of Facts against Heirs-Unequal Division.

I.—*Unicellular Organisms.*

Among single-celled organisms, we know only of heirs-equal division. It is the means by which the species are perpetuated. Were the division a case of heirs-unequal, then there would result, not daughter-organisms of the same species, but daughter-organisms of new species, and all our evidence is against this happening. But although the nucleus divides equally, it is not necessary that the organisms of the new generation be at once like their parent. In many protozoa an ontogeny is gone through. The results of division, for instance, in the fixed Acinetan *Podophrya gemmipara* are free-swimming ciliated forms, which, after a vagrant period, settle down into the fixed adult form. Similarly, among Gregarines quite complicated larval phases are gone through. None the less, in these and similar cases, the exact adult form finally appears. From such occurrences the inference is plain. Because cells are unlike we must not conclude that their nuclear plasm is unlike. To use Weismann's phraseology, some of their determinants may remain latent for a time, and while the complete adult form will appear ultimately, in the meantime the cell may present appearances very different.

II.—*Lower Metazoa and Metaphyta.*

In the case of such organisms as the filamentous Algæ the filament is, so far as can be judged, produced by heirs-equal division. Yet any of the resulting cells, similar to all appearance, may turn into sexual cells. If, with Weismann, it is assumed that the distinction between

somatic and sexual cells, once it has arisen, is inviolable, then we are led to strange conclusions. For instance, in two forms so nearly allied as *Volvox globator* and *Pandorina morum*, we should have to assume that *Pandorina* had no soma, for all its cells can reproduce: while in *Volvox*, where certain cells are specialised as sexual cells, there is a soma.

III.—*Reproduction and Regeneration.*

A multitude of cases show that very many cells and tissues contain the possibility of reproduction. Thus, a plant like *Funaria hygrometrica* may be chopped in tiny pieces, and each piece, placed in damp earth, will reproduce the whole plant. Similar occurrences are familiar in cases like the willow, the begonia, or in many coelenterates, worms, and tunicates. It is making a large drain on the imagination to suppose that in so many widely-scattered cases a condition diametrically opposed to what Weismann regards as the normal condition of complete separation between soma and germ-plasma has arisen under special conditions.

IV.—*Heteromorphosis.*

Loeb uses this term to denote the power of organisms, under the stimulus of outer conditions, to produce organs on parts of the organism where they do not occur normally, or the power to replace lost parts by parts unsimilar to them. Regeneration is the reproduction of like parts. Heteromorphosis is the reproduction of unlike parts.

If one cuts off part of the stem of almost any plant, on placing the stem in suitable soil, roots will grow out, although roots are not natural to that part of the stem. The prothallus of ferns produces the male and female organs on the lower side turned away from the light. If a prothallus be darkened on the upper surface, and illumined by reflected light on the lower surface, then the antheridia and archegonia will be produced on the upper surface. Galls are produced under the stimulus of the insect almost anywhere on the surface of a plant. Yet in most cases these galls, in a sense grown at random on the surface of a plant, when placed in damp earth will give rise to a young plant. In the hydroid *Tubularia mesembryanthemum*, when the polyp-heads are cut off, new heads arise. But if both head and root be cut off, and the upper end be inserted in the mud, then from the original upper end not head-polyps but root filaments will arise, while from the original lower end not root filaments but head-polyps will grow. In *Ciona intestinalis* round a slit cut into the body-wall a tubular process grew out, forming a new mouth, while round the base of this, a series of eye-spots, corresponding to the eye-spots round the real mouth, appeared. In all these cases, it is plain that there were present in parts affected the determinants, to use Weismann's term, not only of the normal parts, but also of parts which, under normal conditions, would never have appeared there; and these new parts growing in

the unwonted places bore the normal species-stamp as characteristically as similar parts grown in their normal places. It can hardly be supposed that the architecture of the germ-plasm contains special determinants to be ready for occurrences so casual, especially as these are called into existence by circumstances quite foreign to the normal environment of the organisms. On the other hand, the facts are consonant with Hertwig's belief that, as all division is heirs-equal division, all the species-characters that depend upon cells are latent in every cell.

The experiments of Driesch, Wilson, and Hertwig upon the early stages of developing ova show that heteromorphosis begins with the very earliest divisions of the egg. Thus Driesch, working upon echinoderm embryos, was able to flatten out the stage where there was a sphere of sixteen cells into a flat plate where all the cells were in the same plane. In such a plate, the nuclei of the cells occupied relative positions very different from the normal conditions. Yet from these Driesch obtained normal plutei larvæ. It was, in fact, as if the cells could be pushed about like billiard balls without destroying the future shape and characters of the embryo. Did each cell contain only the determinants that would correspond to the structures that would arise from it under its normal conditions, then change of its normal position would have arrested development. Each cell must, on the other hand, have contained the determinants for all the animal, and have allowed those to come into operation that were required by the new positions into which the cells were forced. Driesch, by separating the first two and the first four segmentation-spheres of an *Echinus* ovum, obtained two or four normal plutei, respectively one-half and a quarter of the normal size. Here again each sphere must have contained all the determinants for the whole organism. Heirs-equal division must have occurred. So, also, in the case of *Amphioxus*, Wilson obtained a normal, but proportionately diminished, embryo with complete nervous system from a separated sphere of a two- or four- or eight-celled stage.

Hertwig himself, some years ago, published the results of experiments he made upon the development of frogs' eggs under abnormal conditions. He showed that there could be no question of imperative divisions separating the germ-plasm into right and left halves, and so forth, but that the method of division was determined by pressures and relative gravities. Alteration of these made the ova divide into novel but symmetrical forms. Chabry obtained normal embryos in cases where some of the segmentation-spheres had been artificially destroyed.

These cases all show that in its possibilities each segmentation-sphere is identical: that as a result of heirs-equal division, each cell contains all the material necessary to cause the development of a complete embryo. Weismann would have to suppose that in all these cases, in addition to its half of the nuclear matter resulting

from heirs-unequal division, it had also a stock of unaltered germ-plasm ready to be called into activity by unwonted stimuli. But even this hypothesis would not account for cells distorted by compression responding with the production of unwonted symmetries.

V.—*Vegetative Affinities.*

It will now be clear that Hertwig considers that all cells, in addition to their apparent characters, possess, in addition, all the characters of the species.

For instance, histologically considered, the similar tissues in a group of animals such as the mammalia, are, in most cases, not to be distinguished. No doubt an expert could, in some cases, distinguish through the microscope the red blood corpuscles of certain mammalian groups from each other; but he could not do so in the case of species, while with most tissues—connective tissues, nervous tissues, muscular tissues—it would be only in rare and extreme cases that he could distinguish one mammal from another.

But, in addition to their visible structure, which bears an obvious relation to their function, Hertwig attributes to each cell hidden constitutional or specific characters. This is the case plainly enough with the sexual cells. From indistinguishable ova and spermatozoa, there arise animals with different specific, generic or even family characters. But the facts of translation and transfusion for animals and grafting for plants, show that there is a parallel between tissue-cells and sex-cells so close as to suggest that tissue-cells are as much specific as sex-cells.

In plants, the operation of grafting is very easy to bring about; yet it is found that the approximated shoots will not always unite; if the cases where grafting succeeds and fails be considered, it is found that they are parallel to success or failure in sexual crossing. Near allies will unite, but failure follows the attempt to bring together shoots from different species. There are, it is true, some notable exceptions—cases where plants, considered closely allied, will not unite, and cases where plants, regarded as specifically separate, will unite. Grafting follows, not the apparent outward tokens of resemblance, but the vegetative affinities of the cells. It is their latent constitutional characters, and not their patent tissue characters, that are at work. It is upon the experiments of Voechting that most of these conclusions depend. He found in the case of the pear and the apple, which belong to the same genus, that grafts would not succeed, and in their case sexual crossing will not occur; but the pear and the quince do graft, although these belong to different genera. Hertwig thinks it probable that sexual affinity and vegetative affinity depend upon the same ground characters of the cell.

Voechting calls the grafting of twig and stem *harmonic* when a full individuality is reached, *disharmonic* when this is not the case. When the disharmony is complete the two either poison each other

or develop separately. When, however, an intermediate condition exists, growth together may go on for a time. Then, after a time, the twig grows rootlets for itself, which penetrate the stem of the other, and, trying to achieve its own individuality, becomes a parasite instead of a part of the other plant. Sometimes plants, between which disharmony exists, may be brought together by a third. Thus plant A, which will not grow upon B, may have a third plant C, such that when C is grafted upon the stem of A, then B can be grafted upon C.

Resembling the conditions of vegetative affinity are the conditions of sexual union. Thus, infertility is graded from being complete to partial. Sometimes only the first cleavage occurs; sometimes a gastrula and so forth, but death ensues from no outward cause.

Animal parallels, such as transplantation, are rarer. Trembley succeeded in cutting two specimens of *Hydra fusca* across and getting, in one case, the cut halves of the different animals to unite. But he could not graft upon *Hydra* part of another hydroid.

Ollier succeeded, after removing a part of the periosteum from one spot in an animal, in making it grow and become bony in another part of the same animal. But it was an evanescent success, for the new growth being a foreign object in its novel place, speedily became absorbed. In the case of transplanting from the dog to other animals like cat, rabbit, etc., either the new growth got absorbed, or it festered, or became enclosed in a cyst.

Paul Bert transplanted part of the tail of a young rat to another part of the body. There it grew, all the tissues but the nervous developing well. Transplantation of the tail to other animals was more difficult. Usually it festered, and nearly killed the animal. Sometimes it was resorbed; only in cases of very nearly-allied animals did it grow.

A. Schmitt recently found that pieces of skin transplanted to animals of another species invariably festered out, or were quietly resorbed.

In the case of transfusion of blood, failure always was the result of experiments between animals of different species. In large doses it was fatal; in small, harmful; and always, in a very few minutes, the transfused blood corpuscles began to degenerate, their hæmoglobin appearing in the plasma. Landois and others conclude that the results of transfusion correspond with the anatomical affinities of the animals experimented upon.

Hertwig concludes that the cells, in addition to their tissue characters, have species characters, and that as one can speak of the sexual affinities of the sex-cells, so one can of the vegetative affinities of the tissue-cells.

Before proceeding to give an account of Hertwig's further criticism of Weismann's theory of heredity, it is worth while to sum up the part of the argument already treated. Upon Weismann's

theory, the part of the germ-plasm required to direct an individual ontogeny gets broken up and distributed to the various cells of the embryo by a series of heirs-unequal divisions. By this means the cells of the soma come to contain only the determinants which can produce cells of their own order. The exceptions are not numerous. There is, first, the great exception by which unaltered germ-plasm is passed along the germ-tracks to the place of origin of the sexual cells. Next there are a few special cases of adaptation to the needs of regeneration and bud formation in which a certain amount of reserve nuclear matter is retained. Against this Hertwig asserts that all cell-division is by heirs-equal division, and, therefore, every cell, in addition to its patent characters, retains the power of producing on emergency all the species-characters. He goes on such facts as these. In single-celled organisms all cell-division obviously is heirs-equal division. In the lower animals and plants, almost any cell, when occasion demands, may become a sexual cell. Regeneration of lost parts, and still more heteromorphosis, show that each cell is not limited in its capacity to its predestined sphere of work, but can reproduce totally distinct parts of the organism, and can reproduce them in such a way that the specific characters are maintained. The results of grafting, transplantation of tissue, and transfusion of blood, show that cells beside their patent tissue-characters contain latent species-characters, and that, generally speaking, the resemblance between sexual affinities and vegetative affinities shows that tissue cells are as equally specific as sexual cells.

Considerations Against the Doctrine of Determinants.

With his doctrine of heirs-unequal division, Weismann has united his theory of determinants in the germ-plasm of the fertilised egg-cell. Each independently varying part of the adult is represented by biophores united into a determinant. The various determinants are arranged in the plasm or are endowed with such powers that they come liberated at the proper time to enter into the cells to form the determinate. Hertwig's criticism of this, and his arguments against it, are strongly epigenetic.[1] In the modern, as in the older, doctrines of preformation, he sees the same confusion between sequence and causation. If the ancestor of the whole chain of animal-life were an amœba, it would be an empty and meaningless phrase to say that the amœba contained in any real sense the material germ of all the subsequent animals that arise from it. Similarly, although all the cells of the body have their origin from the egg-cell, it is no necessary consequence that the material germs of all subsequent tissues and cells are contained in the egg-cell. The egg and the adult do not form a closed chain of forces. Many other conditions must be present.

Every organic development depends, for instance, upon the absorption and metamorphosis of nutritive materials. The cells grow

at the expense of material taken from the outside, and what in one stage is raw material becomes in the next the starting point of the next cell-generation. Thus food-yolk enters the cells, and, though the difference is only quantitative, produces a qualitative change in the embryo. If one thinks of the number of stages between the egg and the adult, and that each stage has had a large absorption and change of foreign material, it gets plain that it is an error to put into the egg all the incipia of adult structures, as many of these come into existence only at later stages of development.

A second error in the theory of determinants is to attribute to cells—as, for instance, to the ovum and spermatozoon—properties which are not cell properties, but those of combinations of cells. Of the characters of animals and plants, many are due to the co-operation of the whole organism, others to single organs or to groups of cells. If these are to have physical carriers in the germ, are the carriers biophores or determinants? Hertwig can admit for a cell only characters peculiar to cells. A sexual cell can contain physical incipia for chondrin, ossein, pigment, chlorophyll, nerve fibrillæ, but not for the formation of a definite spinal ganglion, a hair, or the biceps muscle. The incipia of these must be groups of cells.

The egg is an organism which, by feeding, growth, and division, can split up into exactly similar organisms, and it is first through the inter-relations of these new organisms that in each stage of development the combined organism makes its stages of growth. Criticising the pangenes of De Vries, Weismann says that there cannot be zebra-stripe pangenes, and so forth. There could be black and white pangenes, for these are cell properties; but the zebra stripe and many similar characters depend not on cells but on the arrangement of cells. Hertwig accuses him of falling into precisely the same error in his determinants. There cannot be in a cell the determinants of organs or structures which depend not on cell characters but upon the relations between cells.

Thus the human state depends upon the co-operation of many classes of men. Supposing, for the argument, all men to have come from a single pair, this pair, according to Weismann, would contain the determinants of the whole state. But the co-operation of individuals into groups, and of groups into the state, is something new which could not exist while there was only a single pair. Of course the subsequent results are based upon the nature of men, but in no mechanical fashion are they contained in the single pair. So far as causal relations exist between the egg and the organism, the parallel holds exactly. There can be no material particles present in the egg as the incipia of characters due to the co-operation of cells. As Naegeli says, to understand heredity we do not require a separate individual symbol for each difference caused by space, time, and property, but we require a substance which, by the linking of its limited elements, can represent every possible combination of differ-

ences, and through permutation can pass into other combinations. As Hertwig puts it, the hereditary material contained in the egg and spermatozoon can be made up only of those material particles which are the bearers of cell properties. Every composite organism can inherit its characters only in the form of cell characters. The numerous indefinitely variable properties of animals and plants which are expressed in the different form, structure, and function of their different organs and tissues, and in the different relations between them, are functions of cell complexes. They depend on the interaction of many cells, and cannot have as their hereditary bearers material particles in a cell. They are new structures which come into existence only through the combination of cells with different individual properties.

Take as an example of the impossibility of determinants, the early stages of the frog's development.

In cleavage the nucleus plays the chief part, but to start this there is needed not a special determinant, but the co-operation of all the functions of the cell through assimilation of nutriment from the yolk. The chromatin fibrils, which we can represent as independently growing and dividing elements, have to be doubled in number. The centrosome also must be independently doubled. All these processes are the result of chemical and other changes depending on the powers of the cell as an elementary organism, and extended to the chromosomes as units of a lower order. The division of the nucleus into two, four, eight, and so forth, gives the stimulus to the yolk-mass to divide, and this exercises a strong influence upon the arrangement of the cells and the direction of the planes of cleavage. But these are not due to special determinants, for many of them are due to the special properties of the yolk. Thus Hertwig himself has shown that the direction of the planes of cleavage depends upon the relations of the specific gravities of the various parts of the cell, and that the unequal size of the cells and the unequal rate at which they divide depend upon the separation of the protoplasm into parts richer and poorer in yolk granules. No doubt the first three cleavage planes often coincide with the future axes of the animal, and this is laid hold of by Roux and Weismann as proof of their view that these planes separate cells containing material for the right and left halves, and so forth. But these also Hertwig thinks he can show to result not from specific determinants, but really to depend upon the shape of the egg and the yolk distribution. Thus the shape of the egg itself in many cases determines the necessary shape of the adult. In the case of eggs, those with polar arrangement of yolk give rise to polar segmentations, those with equal distribution of yolk give rise to holoblastic division. In meroblastic eggs the richer or slighter assembling of yolk, and therefore of specific gravity, is the determinant of the special kind of embryo. In very many cases where the cleavages give the future axes, these are determined not by the

nucleus, but by the distribution of the yolk and by the shape of the embryo.

The formation of the blastula depends on several factors.

1. The cleavage into four, eight, and sixteen causes a loosening of the cells and the appearance of intercellular spaces.

2. As the cells multiply rapidly they naturally come to be pressed into an epithelial structure.

3. As the outer cells become pressed into an epithelium, the segregation of fluid and arching of the outside cause a central cavity to appear and grow larger.

Where is the room for special determinants? The division is an elementary property of all cells; the appearance of intercellular spaces is a result of cellular and external forces as the nucleus forms an attraction centre round which the yolk tends to arrange itself equally.

Gastrula formation is a result of the co-operation of all the cells, of the inequality in curvature, and of a series of external circumstances. To attribute this to determinants is to turn things round. It is not because certain cells contain certain elements that they become epithelial, digestive and so forth; but it is because, by mechanical and other reasons they come to lie in special spaces, that the corresponding side of their character gets developed.

Hertwig's leading objections to Weismannism are then, first, that he thinks the evidence is in favour of cell-division being heirs-equal division, and that, in consequence, the germ-plasm is not separate from the soma, but all the cells in an organism contain the cell-characters necessary to produce a complete organism. Next, a large number of the characters of organisms depend upon combinations of cells, and so cannot have determinants in a single cell. His view is epigenetic in a double way. Each cell contains all the cell-characters of the organism: the special characters which become active are educed by its environment. Characters which depend upon the co-operation of cells can come into existence only by the combination of ceil-characters after these have been found. In the next number of NATURAL SCIENCE I hope to give an account of the concluding portion of Hertwig's pamphlet.

P. CHALMERS MITCHELL.

III.

The Parasites of Malarial Fevers.[1]

NO one to whose lot it may fall to have to travel alone in tropical countries can afford to neglect the study of the various diseases there prevalent. This is necessary not only for self-protection and self-cure, but for sake of the influence that may be thus acquired over the natives by a rapid cure or sudden soothing of pain. Of all the diseases which it is necessary for such a one to consider, there are none more important than those of that puzzling series grouped together as malaria. For long it was thought that many ailments included under this name had no connection, for their causes were quite unknown. During the last fifteen years, however, great advances in the knowledge of the subject have been made. England has taken but a very small share in this, as the disease is not one often met with in ordinary British practice. It is true that in the lower parts of the eastern counties there is a good deal of a malarial ague. A "West Coast" captain recently remarked that the malaria on parts of the Essex coast is as bad as it is anywhere in Africa; but this need not be taken seriously, and only serves as a further illustration of the fact that a West Coast captain will say anything. In the tropics, on the other hand, the disease is one of the most important, but the conditions for its study are not as favourable as might be wished. In Italy, however, both the disease and skilled investigators occur together. There malaria is so prevalent and assumes such a pernicious type, that it has been a matter of study since the earliest days of medical science. Thus, even in the time of Hippocrates, so closely had it been watched that the fevers were then divided into two classes.

The literature of recent work upon the subject is very scattered, but a general sketch of the whole subject has been given in two monographs, of which a translation has been issued by the New Sydenham Society as the 150th volume in its series. These treat the subject from somewhat different points of view. The first is rather

[1] Two Monographs on Malaria and the Parasites of Malarial Fevers. New Sydenham Society. 8vo. Pp. xxvi. and 428, six plates, three charts. London, 1894. I.—"On Summer-Autumn Malarial Fevers." By E. Marchiafava and A. Bignami. Translated from the Italian by J. Harry Thompson. II.—"The Malarial Parasites." A description based upon observations made by the author and by other observers. By Julius Mannaberg. Translated by R. W. Felkin.

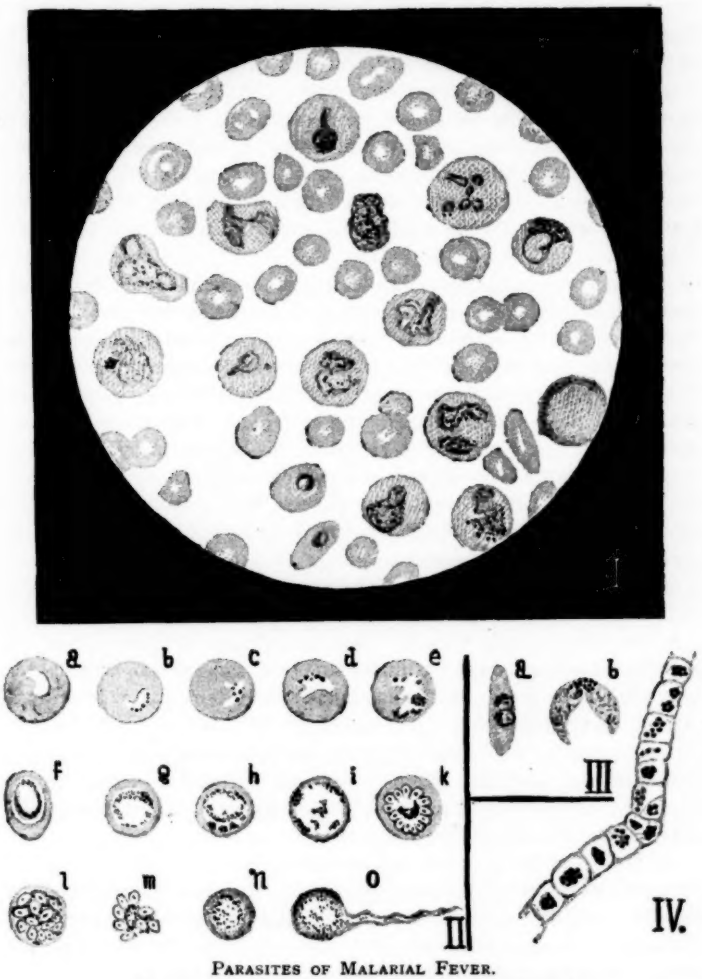
medical in its treatment; it is a study of the worse group of the two groups of fevers of the Roman Campagna: it is entitled "On Summer-Autumn Malarial Fevers," and is by Professor E. Marchiafava and Dr. A Bignami. The second, mainly biological, is on "The Malarial Parasites," and is the work of Dr. Mannaberg, of Vienna. The former is admirably translated by Dr. Thompson, of Washington, who has added an interesting preface of his own: not so much can be said for the translation of the second part of the volume, where misprints, errors in translation, and unintelligible sentences detract from the value of the work.

The study of the subject has an important bearing on some biological questions. It may be of interest, therefore, to attempt briefly, in non-medical terminology, to summarise some of the results for the benefit of general students of biology.

The dawn of modern work on the subject began with Heinrich Meckel's (1) discovery in 1847 of pigment in the blood of a dead patient who had suffered from malaria. The significance of this, however, was not seen till, in 1879, Marchiafava and Celli began their brilliant course of investigations into malaria, by proving that this black pigment or "melanin" was formed within the red blood corpuscle from the hæmoglobin as a product of disease. The next year the great modern impetus to the study was given by the announcement by A. Laveran (2) that he had seen a flagellate organism in the blood of a patient suffering from malaria, which he maintained was the cause of the disease. From that time the study of the subject has made great strides. Laveran appears to have rather jumped at conclusions, and these were therefore not accepted at the time by many of the most careful investigators; thus Marchiafava and Celli dismissed these parasites as merely the products of the decomposition of the blood corpuscles. Nevertheless, the truth of Laveran's main conclusion has been verified, and it is now undoubted that the various types of malarial fevers are due to organisms in the blood.

As in our English ague, the first and most striking feature in these malarial diseases is their recurrence at certain regular intervals. In the old days this could not be properly explained, but it is now known to be due to the fact that the malarial parasites pass through a regular life-history. If the blood of a malarial patient be examined with suitable precautions, with the best type of modern instruments, it will be found to present somewhat the appearance shown in Fig. I., which is magnified a thousand diameters. There it will be seen that many of the blood corpuscles contain foreign parasitic bodies: these are either small and rounded, and occupy only about a tenth of the corpuscle, as shown in one at the lower margin of the figure. These are immature parasites, and their development may be illustrated by the series shown in Fig. II. Here the round discs represent the blood corpuscles: in *a* we see the parasite in its most primitive condition, living inside the corpuscle. It feeds upon the material of its host, and secretes the

red colouring matter or hæmoglobin which it takes in, as small scattered grains of a black pigment or melanin: further stages show the growth of the parasite and increase in the amount of pigment, until in Fig. *i* the whole corpuscle is occupied by the parasite. Nutrition is thus stopped and spore formation commences. In *l* this process is com-



PARASITES OF MALARIAL FEVER.
(For explanation, see end of Dr. Gregory's Paper.)

plete and the next figure shows the free spores. The last two figures of the series show a less normal development that sometimes takes place: here the whole parasite is set free by the rupture of the blood corpuscle. The particular species illustrated in Fig. II. runs its life-cycle in three days, and therefore produces the so-called quartan

fever in which, after one paroxysm, the next occurs on the fourth day. If the blood of a patient be examined on the first day in a case where the fever has a single origin, then only such forms as those represented in Figs. *a*—*c* will be seen; on the second day will be found the forms with the larger hæmatozoa—as the parasites may be called—such as those shown in *h* and *i*. During this period the patient does not suffer fever: if the disease be of the intermittent type the temperature will be normal; if remittent, it will be a little above this point. Then, if the blood be watched, the spore formation (Fig. *k*) is seen to commence, and the temperature of the patient rises, probably by the secretion of some poisonous product into the blood. The paroxysm of the fever, during which the temperature may rise to 106° without serious danger, is always approximately synchronous with the setting free of a fresh generation of spores.

Numerous deviations from this occur: thus some fevers are quotidian and the hæmatozoa complete their life-cycle in one day, and others are tertian with a 48 hours' cycle. Moreover, in any type, many of the hæmatozoa do not complete their normal development. The most important series of irregularities, however, is due to the presence in the blood at the same time of more than one generation, or more than one species of parasite. This is seen in Fig. I. Thus in the lower part of the figure is a corpuscle with a hæmatozoon in the earliest stage, while scattered over the field are several fully developed and two which are undergoing spore formation. Owing to the presence of this latter series the patient is either in or very near to a paroxysm of fever. Twenty-four hours later the younger generation will have reached the spore-forming stage, and thus another paroxysm of fever will be brought about. Therefore, though the life-cycle of this parasite takes two days for its completion, the patient will have a daily attack of fever, as the two generations alternately reach the spore-forming stage.

Considerable difficulty has thus been introduced into the study of these malarial diseases, owing to the complications caused by the simultaneous presence of two or more generations of the same species or even of altogether different species.

This raises the question as to whether the various hæmatozoa are definite species or only forms of one or few species modified by different conditions. This has given rise to great difference of opinion. Laveran and his school maintain that there is but one species and that all the varying forms are due only to polymorphism; thus Laveran sums it up in his great monograph (3), "Du paludisme et de son hématozoaire," issued in 1891, that "Le parasite est unique, mais son évolution est variable." The Italian school, however, and Dr. Mannaberg agrees with them, maintain the plurality of the species; Grassi and Feletti defined two genera, *Laverania* and *Hæmamœba*, and numerous species, and these are now generally

accepted. Mannaberg gives an interesting summary of the discussion and of the observations and experiments whereby it has been proved that each fever is the result of its own special parasite, and that by no known means can one species be changed into another.

Now that the life-history of these parasites has been worked out their true zoological position becomes a matter of interest. Laveran at first assigned them to the vegetable kingdom under the name of *Oscillaria malariæ*; but this view was promptly abandoned by its author, and he renamed the species *Hæmatozoon malariæ*. Numerous other names have been suggested, such as *Hæmatophyllum*, *Hæmatomonas*, *Cystosporon*, *Hæmogregarina*, *Hæmoproteus*, *Polymitus*, etc., but the name most generally accepted is *Plasmodium*, by far the most inappropriate. Medical men usually refer to the parasites as "malarial plasmodia." It need hardly be pointed out to biologists how utterly inappropriate this name is; a plasmodium is an irregular mass of protoplasm *containing many nuclei*. The figures, reproduced here, show that these bodies have but a single nucleus. As Laveran's name, *Hæmatozoon*, was not defined with sufficient precision to allow of its adoption in zoology as a generic term, Mannaberg wisely proposes to use it as a general name for the parasites.

In regard to the class to which they must be referred, numerous opinions have been held: of these several merit notice. Metschnikoff assigned them to the Sporozoa, and placed them in the same group as the genus *Coccidium*. Antolisei, however, referred them to the Gymnomyxa, while Grassi and Feletti considered them to belong to the subclass *Amœbeæ* of the class Rhizopoda. Kruse, again, referred them to the Sporozoa, and to the class of Gregarines. Danilewsky (4) finally, in agreement with the most generally received opinion, placed them in the Sporozoa, but established for them a new class, the Hæmosporidia. Mannaberg, after a careful discussion of the subject and comparison of the characters of the parasites with those of the allied forms of Protozoa, agrees with Danilewsky. He divides the Sporozoa into five subclasses, the Coccidia, Gregarina, Myxosporidia, Sarcosporidia, and the new subclass of Hæmatosporidia.

The results of these researches are not only of interest biologically but of great practical value, for with a knowledge of the cause, it is possible to apply remedies with greater certainty than of old. One of the most interesting sections in each of the two monographs is that dealing with the action of quinine. This has long been known as the most efficaceous of all medicines, but its power has been assigned to very different methods of action, as, for example, to a stimulation of the nervous system. Binz (5), however, in 1867, laid the foundation of a sounder view by showing that, if Infusoria be subjected to the action of quinine, they are killed, and, further, that this is due to asphyxiation: the alkaloid destroys the power of the Infusoria to take up oxygen. The quinine acts on the Hæmatozoa

in the same way; it therefore acts only when the parasites are passing through the stages of nutrition and development. When all the hæmoglobin of the corpuscle which acts as the host has been converted into melanin, the parasite ceases to grow, but divides into spores for reproductive purposes, and quinine becomes powerless. It is therefore necessary to take quinine in the intervals between attacks of fever, and useless to do so when the pyrexia is at its worst. This was long ago determined empirically by tropical practitioners, who also understand that quinine really does as much harm as good if taken at the wrong time. The reason for this also is now explained, for Binz has also shown that quinine has an unfavourable action on the leucocytes, or white corpuscles, which are thus checked instead of being aided in their struggle with the disease. Thus an unintelligent use of this reagent may be attended with very unfavourable results. This is likely to happen as many of these malarial parasites are not checked by quinine: this is the case with those which cause malarial fever in birds, and with some forms of human parasites. For instance, those shown in Fig. III., and known as the "crescentic bodies of Laveran," are quite uninfluenced by this reagent. If microscopic examination be neglected, and quinine administered in strong doses under the idea that the fever is produced by the species illustrated in Fig. I., serious injury may result.

The researches embodied in these two monographs are of the highest interest, both to the biologist, from the detailed investigation of the life-history of these low Sporozoa, and to the doctor, from their guidance in the proper treatment of a very troublesome and obscure group of diseases.

EXPLANATION OF FIGURES.

I.—Blood infested by malarial parasites (after Mannaberg), magnified 1,000 diameters. It shows (1) normal red blood corpuscles (the round discs with white centres); (2) a normal white corpuscle or leucocyte (the large dark circular mass touching the lower right hand border of the figure); (3) red corpuscles infested by the parasite in its youngest stage (two shown one above the other, in the middle line, just above the lower margin of the figure); (4) red corpuscles almost fully developed (the large discs containing shaded bodies); (5) parasites in which spore formation has commenced (one occurs in the upper part of the figure, a little to the right of the middle line; it is a dark body, roughly oval in form.)

The hæmatozoa belong to a tertian form. Thus, as spore formation is just beginning (as shown by the large size of most of the older generation of parasites, and that in two spore formation is fairly advanced), the patient is approaching an immediate paroxysm of fever. As a second generation is also present this will cause another attack of fever next day, so the patient is suffering from a "double tertian fever." Quinine if taken now would kill the parasites of the second generation, have no influence on the older generation, and check the efforts of the white corpuscles and of the phagocytic cells in the spleen to eradicate the disease.

II.—*a–o*, the life-cycle of the hæmatozoa of a quartan fever. (After Marchiafava and Bignami). In *a–l* the parasites are shown in red blood corpuscles. In *a* the parasite is in its simplest unpigmented condition; *b–i* show the gradual

growth of the parasite and increase in the amount of pigment; *k* and *l*, spore formation; *m*, free spores; *n* and *o*, free pigmented degenerate forms.

III.—The "Crescentic bodies of Laveran." (After Mannaberg). Magnified 1,000 diameters. In *b* transverse segmentation is shown.

IV.—Capillary vessel from the brain of a patient dead from malaria comatosa; the capillary is choked by spore-forming bodies.

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J. W. GREGORY.

IV.

A British Palm.

THE mention of Palms carries us, in imagination, to a warmer climate than our own, as we figure the tall, stately, unbranched stem crowned by a few large spreading leaves. But all are not lofty; in some cases the trunk is very short, or there is only a stout underground stem terminating at the surface of the soil in a leafy crown; while a *Thrinax* recently discovered at Anguilla, in the West Indies, generally reaches only about a foot in height—a specimen 2 ft. 8 in. being described as quite tall—and a dwarf species of *Phœnix*, the genus to which the date-palm belongs, discovered shortly before in the East Indies, was likewise only about 25–30 in., forming, with its slender stem, a very attractive little plant.

Others are not even self-supporting, but climb or scramble over surrounding vegetation. Such are many of the tribe Calameæ, which includes the Rattans or Cane-palms (*Calamus*), with a very long slender stem, stated by Blume, in his "Rumphia" (vol. ii., p. 158), on the authority of Rumphius himself, to reach a length of 1,200 to 1,800 feet. This statement has not, however, been verified, though 300 feet is said to be a common length in Ceylon and the Malay archipelago.

The leaves of the scramblers often terminate in long appendages or flagellæ, armed with stout recurved hooks, which may be extremely formidable and dangerous, though of great service to the plant as holdfasts. In one of these, by the way, *Korthalsia scaphigera*, from the Malay peninsula, the stipules of the leaf are united and swollen, forming an oval, hollow, smooth-walled chamber, of which ants take possession as a home.

According to the "Genera Plantarum" of Bentham and Hooker, there are about eleven thousand species, many imperfectly known; while many more, without doubt, remain to be discovered in Africa, Central America, Madagascar, and the islands of the Pacific Ocean. The family is essentially a tropical one, the greater number being American, a few Asiatic and Australian, and very few African. In America, a few small genera are peculiar to the Southern United States, such as *Serenæa*, dedicated to the late Sereno Watson by Sir J. Hooker, and found in Florida and Carolina; *Washingtonia*, inhabitating South California and Arizona; and

Erythea, peculiar to the former. In South America, *Jubæa*, a Chilian genus, reaches as far south as the 37th parallel of latitude. The only species, *Jubæa spectabilis*, is popularly known as the Chili Coco-nut, and its fruits, looking like miniature coco-nuts an inch and a quarter long, are sometimes sold in this country. In the Western Hemisphere, the southern limit is 44° south latitude in New Zealand, the extreme form being a small endemic palm, classed originally as an *Areca* (one species of which, *A. catechu*, yields the well-known Betel-nut) but now separated, together with a closely-allied plant from Norfolk Island, as a distinct genus, *Rhopalostylis*. North of the Equator the monotypic (*i.e.*, containing only one species) genus *Nannorhops* lives on the mountains of India, beyond the Indus, and extends through Afghanistan and Beluchistan to S.E. Persia. The only native of Europe is a species of *Chamærops*, a small genus of dwarf palms confined to the Mediterranean region. *Chamærops humilis* is found in the south of Spain and Portugal and on the Mediterranean seaboard, reaching its northern limit at Nice at a latitude of 43° 44′. It is generally quite a dwarf, only three or four feet in height, and sends up numerous suckers in dense tufts. It may, however, form a trunk twenty or thirty feet high. Nyman, in his "Conspectus Floræ Europæ," speaks of it as the lowly and degenerate representative of a noble family afforded by the European flora, but reminds us that in the extreme south a loftier palm has found a home, viz., *Phœnix dactylifera*, the date palm. Beyond 38° or 39° latitude the date becomes merely an ornamental tree, as its fruit no longer matures, remaining more or less acid to the taste. It is cultivated in great quantity over the whole of North Africa, and more sparingly in Western Asia and Southern Europe. Like wheat, the sugar-cane, and many of our ancient useful plants, it is now never found in a wild state, and its original home is unknown.

Of course, pampered individuals in favoured spots will thrive in still more northerly latitudes. The Chusan palm (*Trachycarpus excelsa*) is sufficiently hardy to stand the winter in the north of France and even in the Isle of Wight. It has, however, a protection from the cold in the form of a coarse hair resulting from the decomposed bases of the leaf-stalks. The ordinary ratepayer may become acquainted with the family in the London parks during the summer months, where, by the way, I recently heard one described as "the kind of tree of which you see pictures with missionaries sitting under." It was labelled, incorrectly, *Seaforthia Cunninghami* in small letters and low down. "An Australian Palm" in larger print would have conveyed more information to the author of the remark.

The one hundred and thirty-two recognised genera of palms are somewhat restricted in their distribution. The *Raphia*, indeed, is a native of both Old and New Worlds, having several species in tropical Africa and Madagascar, and one in America, from the mouth

of the Amazon as far north as Nicaragua. Several of the tropical Asiatic genera, like *Areca*, run down through New Guinea into Australia. Others are extremely local. Lord Howe's Island has, for instance, two peculiar genera. *Juania* is found only on the small island of Juan Fernandez, while the small group of the Seychelles, in the Indian Ocean, boasts no less than five endemic genera. One of these, *Lodoicea*, bears the fruits known as the double coco-nut, or *coco de mer*. The great woody nuts were found floating in the sea long before the discovery of the tree which produced them or the islands on which it grew, and many absurd stories of their origin were in vogue among the earlier navigators.

Of somewhat restricted distribution, as well as of doubtful relationship, is the *Nipa* palm. Only one species is known, *Nipa fruticans*, which frequents the brackish estuaries and littoral marshes of tropical Asia from Ceylon to the mouth of the Ganges, through the Malayan Archipelago to the Philippine Islands, New Guinea, and tropical Australia. It is of dwarf habit, having a long, thick horizontal root-stock which sends roots into the ground and bears, at the apex, a number of long deeply-cut pinnate leaves. The male and female flowers are borne separately on a spadix or long fleshy stalk springing from among the leaves. The male are thickly crowded on catkin-like branches, the female borne in a rounded head terminating the spadix. The head of ripe fruits is as large or larger than that of a man. The individual fruits vary somewhat in size and shape, according to their position in the head, but are all more or less inversely egg-shaped, the upper-third projecting and rounded with generally an apical umbo, the lower portion pressed on all sides by neighbouring fruits and more or less angular, showing four to six obtuse angles. The coat is of a deep brown colour and polished, while the substance is thick and fleshy but densely fibrous. It contains a single seed with a small basal embryo and a quantity of cartilaginous albumen, which, however, is edible before the seed is quite ripe. The seed is said to germinate within the fruit, which remains on the spadix for several years, by which time the embryo has so far advanced in growth as to be unaffected by salt water. I think, however, that this statement requires confirmation. The fruit finally drops into the brackish water or mud and the seedling may grow where it falls, or float some distance. Great numbers of the fruits are often found floating in the river estuaries. Sir Joseph Hooker, in his delightful "Himalayan Journals," refers to the frequency with which they were tossed up by the paddles of his steamer in the Sunderbunds.

In the "Philosophical Transactions" of the Royal Society for 1757, Dr. James Parsons gave an account of some fossil fruits and other bodies found in the London Clay, in the island of Sheppey, by Edward Jacob, a surgeon of Faversham, who also subsequently published an account of them as an appendix to his "Plantæ Favershamienses"

in 1777. Of two of the fruits, figs. 1 and 3 on plate xv. of the Royal Society paper, Parsons says they seem to be figs petrified when hard and green. In 1785, James Douglas, in a work entitled "A Dissertation on the Antiquity of the Earth," refers to the Sheppey fruits, one of which he makes out to be "a species of almond," but somewhat qualifies his determination when he further characterises it as "remarkable in the opinion of the most intelligent persons, as bearing no analogy in size to any recent specimens of this nut discoverable in any quarter of the globe." This particular specimen was the property of Sir Joseph Banks, and is probably identical with one of the two figured nearly twenty years later by James Parkinson, a London surgeon, the author of "Organic Remains." By this time the fossils, through Sir Joseph Banks and Mr. Douglas, had become the property of the British Museum. In the "Organic Remains," the green figs and the species of almond become allied to the coco-nut, Parkinson suggesting that they may, perhaps, be referred to the genus *Cocos*.

A year before the publication of Douglas's "Dissertation," Burtin, in his "Oryctographie de Bruxelles," had already described as coco-nuts some large fossil fruits found in beds of Middle Eocene age at Woluwe near Brussels. Others have since been brought from the same locality, and may be seen along with the Sheppey fossils in the geological department at the Natural History Museum; they are as large as coco-nuts, but broader and more egg-shaped.

None of these fruits received specific names till 1828, when Adolphe Brongniart, in his "Prodrome," accepting Parkinson's generic determination, dedicated the Belgian ones to Burtin, as *Cocos Burtini*, and those from Sheppey to Parkinson, as *C. Parkinsonis*.

In 1840, James Bowerbank, who had accumulated an enormous number of fossil plants from Sheppey, brought out the first and only part of his "History of the Fossil Fruits and Seeds of the London Clay." The first portion of this book is devoted to a discussion of the affinities of the particular specimens above-mentioned and others more or less resembling them. From a careful study of a large number, Bowerbank was led to reject the idea of a close alliance with the coco-nut, and also the name *Pandanocarpum*, applied to some of them by Brongniart, to indicate what he considered to be their great analogy with the fruits of *Pandanus*, the Screw-pine. As Bowerbank points out, the fruits are never aggregated into portions containing several seeds as occurs in *Pandanus*, while on the other hand they closely resemble those of *Nipa fruticans*. In both cases we find the thin outer skin, or epicarp, the fibrous central portion, or mesocarp, and the somewhat indurated internal endocarp, protecting the single seed. Each is in fact a drupe constructed on the same principle as the plum or almond. The fossils never show any indication of the presence of three carpels, either by three pores as in the modern coco-nut or otherwise. To indicate their affinity with the modern *Nipa*, Bowerbank founded the genus *Nipadites* and went

on to describe no less than thirteen species. So close is the resemblance to *Nipa*, that though widely separated from its present area of distribution, there is little doubt but that the Sheppey fruits belong to that genus of Palms. In fact, I was greatly tempted in a recent monograph, which the Linnean Society did me the honour to publish in their Journal, to follow Ettingshausen, who makes them all species of *Nipa*. The suffix "ites" has, however, been much used in palæontological work, and as there must always be some uncertainty in the specific diagnosis of fossils, based, as it often is, on material which would scarcely be considered sufficient in the case of recent plants, I have retained the form in which it was left by Bowerbank.

As often happens when a man makes a special study of small groups of plants, Bowerbank made too many species out of his fossil palm fruits. Some of these are founded on differences which may be merely individual variations or depend on the degree of maturity of the fruit or its position in the head. It is important to remember that, densely packed as they are in a sphere, there is not only considerable variety in the number of sides to the fruit and the development of the angles, but as many never ripen, all stages of abortion may be found, especially near the base of the head, compressed and distorted by the growth of the ripening drupes. There is every reason to believe that the fossil fruits were borne in a similar way. A comparison of some of Bowerbank's species with a number of immature or aborted fruits, taken from a head kindly sent by Mr. Ridley from Singapore, showed that more than one might be thus accounted for. The thirteen can be in this way reduced to six or at the most seven. One or more of these have also been found near Bournemouth and in the Isle of Wight. Three years ago Mr. Clement Reid, of the Geological Survey, made an interesting discovery on the Sussex coast in the Bracklesham Beds, at Bracklesham and West Wittering. In portions of the beach bared by the effect of a recent gale, he saw embedded a number of large fruits which we found on closer investigation to be *Nipadites* of a much larger size than any hitherto found in this country, and quite equalling those already referred to from the Belgian beds. Mr. Reid has since come upon similar specimens at Hengistbury Head, near Christchurch, Hants. The mode of preservation of these differs from that of the London Clay and Belgian fruits, for while the latter have been rendered hard and solid through the infiltration of mineral matter, our recent finds consist merely of a carbonised shell filled with sand, which rapidly crumbles on removal from the damp sea beach, while the carbon film cracks and peels. Hence it is impossible to keep them for any length of time. One, in a fair state of preservation, is now in the Jermyn Street Museum, and another in the British Museum at South Kensington. Fruits almost precisely similar to these have been found in Italy, near Breonio, in Verona.

Though described under a different name, *Palæokeura*, I have no doubt as to their identity with *Nipadites*.

The conditions under which the fossils are found show that, like their modern representative on the shores of tropical Asia, the palms from which they fell grew in the rich brackish mud of the shore or estuary. Associated with the Sheppey fruits are remains of crabs, fresh-water turtles, and a great variety of fruits and seeds, with stems and branches; such a collection, in fact, as characterises the delta of a large river. The great diversity of forms, and the water-worn condition of many of the specimens, indicate a collection brought together from a large area. The Belgian and Italian fruits must have floated some time, as they are often much bored by the teredo, and the associated fossils have a somewhat more marine character than those from the London Clay. On the other hand, the appearance of the fruits which we saw on the Sussex coast gave the idea that they might have grown close by, so uniform was their size and condition. The absence of teredo-borings and water-wear favours this view, but this being the case, we should almost expect to find traces of the stem or leaves of the palm; these, however, have not yet been found in any locality. Water-worn drift-wood, showing coniferous and dicotyledonous structure, occurred along with the West Wittering specimens.

From the general facies of the flora and fauna in the different beds in which *Nipadites* are found, there is ample evidence, as Bowerbank, Ettingshausen, and Schimper have demonstrated, of the existence of a climate much warmer than the present, and approaching a tropical one, or at least subtropical in character.

A. B. Rendle.

V.

The Bird's Foot.

I.—By Frederic A. Lucas.

I TRUST that the author of the note on the bird's foot, Natural Science for July, page 10, will not consider me "a captious critic" for saying that the statement that "in the great majority of birds. . . . the tendon of the flexor of the great toe is connected by a tendinous slip with the common deep tendon of the other toes" may be a little misleading.

Regarding the groups of birds in a general way, the statement is true, but so far as mere numbers go, the Passeres and Humming-birds, which are schizopelmous (*i.e.*, with the tendon to the great toe unconnected with the common deep tendon), probably contain a majority of species.

Now in regard to the presence of the flexor longus hallucis in three-toed birds, it seems to me that we can bring additional evidence to sustain the proposition that it remains not as a mere rudiment, but because it is functional. In certain lizards—possibly in the majority, or even all—the long flexors of the foot unite beneath the ankle and are worked by a single muscle; and did we know of any bird in which there was no separate muscle for the flexor longus hallucis, this bird would be considered as presenting the primitive arrangement of the tendons.

In the common Chimney Swift of North America (*Chætura pelasgia*) we have a comparatively simple condition of affairs. The deep tendons are united for a considerable distance, and the tendons to III, 2, and IV, 3,[1] arise from a common slip. There is another tendon to II, 1, the total number of separate long tendons thus being four. I, 1, is worked by a separate muscle and tendon arising from the upper inner portion of the tarso-metatarsus, this being present also in other Swifts, in Humming-birds, and in such Passeres as I have dissected.

In another and more typical Swift, *Micropus melanoleucus*, there are separate tendons to II, 2, III, 2, and IV, 2, the deep flexors being, as before, firmly united. In *Trochilus* we have a somewhat intermediate

[1] Roman numerals represent digits; Arabic, phalanges.

condition, the deep flexors being independent of one another, while III, 2, and IV, 3, are, as in *Chætura*, forks of a common tendon, separate tendons running to I, 1, and II, 1. Finally, in a number of passerine birds we have, besides the separate deep flexors, the following tendons, II, 1, II, 2, III, 1, III, 2, and IV, 1, seven long tendons in all. Concomitant with this there are other steps towards increase in what we may call tendinal individuality, for while in the Swifts the tendons playin a groove[2] at the upper end of the tarso-metatarsus, in some, though not in all, Passeres, there are six distinct foramina for the accommodation of the seven tendons.

Now the bearing of these facts on the case under consideration seems to me this: There is an increase in the number and freedom of tendons as we proceed from the lower to the higher birds, and if three-toed birds retain the flexor longus hallucis it is because they stand low in the avian scale and had lost their fourth toe before its tendon became differentiated from the others.

Right here is a point I should like to see explained. Why has *Chauna*—and *Palamedea* also—a thumb and no flexor? Has it lost the flexor through disuse, or has it, which it has no right to do, grown a thumb after having once discarded that digit?

Turning back to Mr. Finn's interesting paper in the June number, there are one or two points which invite a few additional words. The great objection to Darwin's remark that "it might be of advantage to a penguin-like bird first to flap along the surface of the water like the Loggerhead Duck (*Micropterus*) and ultimately rise in the air" is that, so far as I am aware, birds which take to water have their flight impaired, and that the transition is from air to water, and not from water to air. The Loggerhead Duck is said to fly when young and do its flopping later in life. The auks have had their wings much reduced in order to use them in a medium for which they were not originally intended, while the great auk lost his power of flight, and eventually his life, by not taking sufficient aërial exercise.

Lastly, is the foot with all toes turned forward a case of reversion for adaptive purposes, or is it a case of survival? While the supposition that, if birds are descended from reptiles, these reptiles were arboreal in their habits seems to the writer as very plausible, it is also fair to assume that there was a time when all four toes pointed forward,[3] and why may not the cosmopolitan Swifts have retained their toes as they were originally? Opposed to this, however, is the highly modified condition of the phalanges, and the fact that in *Micropus* the tendons are a little more specialised than in *Chætura*, which has the

[2] *Macropteryx* has a single tendinal foramen, but the members of the genus *Macropteryx*, though Swifts, belong, to the best of my knowledge and belief, to a distinct family.

[3] In support of this assumption is the fact that in the embryo the four toes point forward.

foot of an ordinary perching bird; so for the present we may let the question rest.

II.—By Frank Finn.

There are some points in Mr. Lucas's interesting paper on this subject on which I should like to offer a few remarks.

In the first place, Mr. Lucas considers that the deterioration of flight in birds which take to water is the greatest objection to the derivation of flying birds from aquatic ancestors. This would indeed be a powerful argument were such deterioration universal; but it is not. A surface-swimming bird may on the one hand take to diving, and then is pretty certain to deteriorate in flying power. The Anatidæ form a good example of this, the semi-terrestrial Sheldrakes having larger and broader wings than the more aquatic typical ducks, while these again are better winged than the diving-ducks (Fuligulinæ); and the spine-tailed ducks (Erismaturinæ), which seem to be as aquatic as Grebes, have the smallest wings of all, except the Loggerhead, which is an outlying degenerate form with no very great diving power.

But, on the other hand, a surface-swimming bird may take to flying more than ever. It can hardly be doubted that the Gulls are better flyers than even their Plover-like ancestor, though the Alcidæ have, as Mr. Lucas points out, degenerated.

Similarly, the Steganopodes exhibit, in the Pelicans, surface-swimming forms with powerful wings, and the wing-power is increased in the Gannets, Tropic birds, and especially the Frigate birds; while the diving Cormorants have their wings reduced, and the extinct great Shag (*Phalacrocorax perspicillatus*) was apparently becoming flightless when its fate overtook it.

The Phalaropes present us with an interesting instance of water-birds of recent evolution; they are as yet only surface-swimmers, with wings not noticeably different from those of ordinary littoral sandpipers, and may in the future develop either way.

Thus, the transition is rather, in the first place, from land to water, and the bird may either become more aërial or more aquatic in consequence of the change of habit.

Similarly, birds which are markedly terrestrial are either good flyers, as Plovers and Sand-grouse, or exhibit a tendency to degeneration in flight just as marked as that of some aquatic birds. Instances of this are, of course, the Ratites, and such Carinates as *Ocydromus* and *Stringops*. The Gallinæ and Crypturi are also usually poor flyers, though none are yet quite flightless. *Stringops*, be it observed, must have lost its arboreal habits before it became flightless, and we find that the genera of Parrots which are probably most nearly allied to it, *Pezoporus* and *Geopsittacus*, are already terrestrial. Arboreal habits tend to preserve an average type of wing, flightlessness being obviously disadvantageous in such case, and also marked length of

wing feathers.[4] And this average wing is particularly characteristic of the Passerines and of most of their allies, which, as has long been known, present a general structure most marked by the absence of special peculiarities. In certain respects, indeed, their specialisation fairly entitles them to their usually conceded place at the head of the avian tree; but their broad outlines are those of a simple and primitive form. In this respect, as I pointed out in my previous paper, they show some analogy to the Primates among Mammals.

This brings me to my second point. Mr. Lucas suggests that the first birds might have had all their toes pointing forward, and that the Swifts may have retained this arrangement. But the fact, which he points out, and which I implied in my paper, that this arrangement is not universal even with them, tells against this view, as he admits. As to the embryo, I must say that in some nearly mature embryos of the Common Swift of Europe (*Cypselus apus*) which I examined, the hallux did not seem to me distinctly directed forwards.

Besides this, it is noteworthy in this connection that a type of foot with opposable toes is very likely to be developed in consequence of arboreal habits. The Primates almost universally, the Opossums, and *Lophiomys* in the Rodents, have opposable halluces among the Mammals; and though the grasping foot of the Chamæleons is not exactly like that of a perching bird, it shows that a very arboreal reptile can develop a prehensile foot with opposed digits; and I can see no reason why this should not have taken place with the primitive bird-reptile before it acquired the power of flight.

III.—By P. Chalmers Mitchell.

Owing to the opportunities afforded me by the kindness of my friend, Mr. F. E. Beddard, the Zoological Society's Prosector, I have recently been able to devote a considerable amount of attention to the anatomy of the bird's foot, and I am glad to respond to the invitation of the Editor to say something on the interesting discussion raised by Mr. Finn's paper in the June number, and by the editorial note in the July number of Natural Science.

There are two questions which have an important bearing upon each other, but which must be argued out separately. Mr. Finn argued for his view that three-toed birds, so far as the toes are concerned, are degenerate descendants of four-toed birds, and that the avian stock probably arose from an arboreal four- or five-toed reptile. In support of his argument, he brought together the cases in which apparently three-toed birds have been shown to possess a rudimentary fourth toe. In the Natural Science note it was pointed out that

[4] A typical form would be the Jay. Is it not possible that the flight by single violent flaps alternating with sailing often affected by this bird and others similarly winged may represent the earliest form of flight, originating in the effort of a parachuting animal to avoid falling?

even in those cases where the first toe is absent there is present a distinct flexor muscle corresponding in its position and relations to the flexor longus hallucis, the long flexor of the great toe. The tendon of this muscle has, of course, no great toe in which to be inserted, and unites with the common tendon of the deep flexor which splits up to work the other three toes. The writer pointed out that the existence of this muscle appeared to be strongly confirmatory of Mr. Finn's view. Unless three-toed birds originally were in possession of a hallux, why should they have a flexor hallucis? He proceeded to point out that this, however, cannot be taken as the survival of a meaningless vestige. For, in the great majority of avian groups (I accept Mr. Lucas's emendation, and quite agree with him that the words of the note were expressed so that a careless reader might suppose that individuals or species were meant), the flexor tendon of the hallux, before running to its insertion upon the hallux, gives off a stout slip to the common deep tendon of the other toes. Apparently it is this functional part of the muscle that is preserved. Were there a case of a Passerine where the hallux is absent, then we could see whether a functionless rudiment would be retained; for in Passerines there is no slip from the longus hallucis to the common tendon of the other toes.

Mr. Lucas brings forward interesting considerations directed to the view that in birds there is a progressive tendency towards the individualisation of these tendons. He suggests that, as is the case in many existing Lizards, so possibly in primitive birds a single muscle had many tendinous insertions, and that later on the muscle gradually broke up till more and more of the tendinous insertions came to have separate muscular bellies. He suggests, therefore, that in the case of three-toed birds they may have lost their toe before the muscle became individualised, and therefore he thinks that this condition of the tendons means that the birds are low in the avian scale, and therefore also, he concludes that the note does not supply an argument in favour of Mr. Finn's view. The three-toed birds, according to him, do not show evidence of the modification of their foot from a specialised four-toed foot, such as would be possessed by an arboreal form.

The matter is so interesting that I shall elaborate the argument more fully, as I am convinced that it is sound. In a typical four-toed bird there are two deep plantar muscles. The belly of the flexor longus communis lies most closely applied to the posterior surface of the shaft of the tibio-tarsus. At its upper end it spreads out fan-like, and takes origin sometimes continuously, sometimes by a series of digitations, from the upper end of the tibio-tarsus, with occasionally a slip from the femur. Its single tendon runs through a bony canal in the posterior part of the ankle-joint, and then runs down the posterior face of the shaft of the tarsus-metatarsus, near the lower end of which it splits up into a tendon for the second, third, and fourth toes. The flexor longus hallucis lies externally to the belly of the flexor

communis. It is a rounder and narrower muscle, entirely distinct from and unconnected with the flexor communis above the ankle-joint, and arising by a single head or by two heads from the external condyle of the femur, and from the intercondylar region. Its single tendon of insertion passes through the ankle-joint, generally through a separate bony canal which is placed more laterally than the bony canal of the flexor communis. Below the ankle-joint the tendons of the two muscles lie side by side, but in their course down the shaft of the tarsus-metatarsus the tendon of the flexor hallucis crosses over the tendon of the other, and in so doing (as has already been mentioned) except in the Passeres and Humming-birds, it sends a strong vinculum to the communis tendon, and then runs on to its insertion in the hallux. Now whatever may have been their original connection, these two muscles are as distinct and individual as possible. In the case of an ordinary three-toed bird like the Oyster-Catcher (*Hæmatopus ostralegus*), the two muscles are as separate in their origin and course as in a normal four-toed bird; the only difference is, that with the loss of the hallux, the slip of tendon which runs from below the vinculum to the tendon has disappeared. The facts almost irresistibly bear out the conclusion that such a three-toed bird has been derived from a four-toed bird not of a primitive character, but with a foot like existing normal four-toed birds. But evidence—one is almost tempted to call it experimental evidence—is furnished in favour of this view by the anatomy of the foot of *Pterocles*. Whether the Sand-grouse be considered a pigeon or a gallinaceous bird, most will agree that it is in the present condition of losing its great toe. In *Pterocles alchata* there is no trace whatever of the slip from the vinculum to the minute toe. But above that the two tendons, and above the ankle-joint the two muscles, are perfectly distinct; the flexor longus hallucis arises by two heads from the femur, while the flexor communis spreads out over the upper end of the tibio-tarsal shaft. Here is a case of recent degeneration of a hallux and the production of the same type of foot as in ordinary three-toed birds. It seems to me absolutely conclusive in favour of Mr. Finn's view.

But Mr. Lucas's remark about *Chauna* and *Palamedea* calls up another exceedingly strong piece of evidence. *Chauna* has no flexor tendon to its hallux, but in other respects the relations of the flexor longus hallucis down to the vinculum are precisely similar to those of four-toed birds. Mr. Beddard and I recently had an opportunity of dissecting *Palamedea*, and the results of our observations, communicated to the Zoological Society, will shortly be published in its *Proceedings*. We found that there was a separate tendinous slip to the hallux. So here again is a case showing that the mere suppression of the tendinous slip from the vinculum to the hallux changes a four-toed foot into the type of structure found in three-toed birds.

VI.

Notes from the British Association, 1894.

THE British Association is always a medley, and this characteristic is very apparent in the addresses delivered before it and the papers read at it. There are always some people who have something to say and some who have to say something. There are those who have manner without matter, and those who have matter without manner. Its great mission is to interest the public in science. The general public go there to be as scientific as they can. The scientific public go there to frivol as much as they may. It serves many useful purposes, but among these the least is to bring new scientific results before the scientific public. But although one need not expect any large bulk of novel and interesting scientific matter, there are always some interesting messages from the scientific world to the public, and not a few matters of general scientific interest.

We propose to give here, not an account of the meeting that shall pretend to any completeness, but notes on such matters as seemed of special interest to our representatives at the sections. As we have dealt elsewhere with Lord Salisbury's address, we shall pass straight to the sectional meetings.

Neglected Mineralogy.

The President of the Geological Section represents a neglected branch of science, and Mr. Lazarus Fletcher made excellent use of his opportunity in a direct plea for mineralogy by pointing out the lack of opportunity for the study of it in this country, and in an equally cogent indirect plea by the brilliancy of his own exposition.

Having given a Homerically imposing alphabetical list of living German scientists, from Arzruni to Zirkel, whose names are familiar as distinguished mineralogists, Mr. Fletcher went on to contrast the wealth of educational and research equipment of Germany with the apathetic treatment which mineralogy is receiving in the British Isles. In every German university there is a professor of pure mineralogy, while other continental nations are not far behind Germany in their practical appreciation of the subject. But here, throughout Great Britain and Ireland, the imposing number of the professorships in the science amounts to *two*; and in only one of the two universities is the subject allowed to rank with other sciences in the examinations for degrees.

University extension in a mineralogical direction was the theme of Mr. Fletcher's interesting address, and surely such an appeal was never more needed than at the present time when the study of minerals is being more than ever pressed by other countries, and must be undertaken by almost every geological student. Such has been the indifference felt towards mineralogy, that the last report on the progress of the science submitted to the British Association was that given sixty-two years ago by Dr. Whewell, shortly after he had resigned the professorship of mineralogy in the university of Cambridge. It was therefore incumbent upon the president of Section C, as a mineralogist, to give some account of the progress of the science since that date, and of its present condition: this duty Mr. Fletcher performed in the earlier part of his address in a masterly and captivating manner.

The audience was rapidly conducted over the recent history of crystallographic methods and theories up to the parallelepipedal and other arrangements of points which have been devised to account for the structure of crystals, special emphasis being laid upon the notation and methods introduced by the late Professor Miller, of Cambridge. This was followed by a brief survey of the development of optical and physical research as applied to the study of minerals; by a sketch of the remarkable progress of mineralogical chemistry during the last sixty years; and finally by an enumeration of the more important instruments which have been invented and improved during this period and which have enabled the mineralogist to extend his investigations in every direction, and have helped to raise mineralogy to the position of an exact science. No one has by his labours contributed more to this result than the late Professor Mallard, of Paris, whose lamented death last month was alluded to by the President as depriving mineralogy of her greatest philosopher.

If much of the address dealt with things that were absolutely unknown to the majority of his audience, Mr. Fletcher's argument was only the more strengthened, for where, under the present conditions, could they be expected to have received any instruction in the refinements of modern mineralogy, or to have learnt that there is a real science of minerals?

We hope that this address will open the eyes of many people. The words quoted from the Report of Dr. Whewell are strictly applicable to the present day; they related to the lack of "interest with which mineralogy, as a branch of natural philosophy, has been looked upon in England. Indeed, this feeling appears to have gone so far that all the general questions of the science excite with us scarcely any notice whatever. But a more forward and hopeful spirit appears to have prevailed for some time in other countries, especially Sweden, Germany, and more recently France." And yet "the step to which mineralogy owes the best portion of its scientific character was made by an Englishman, the Doctrine of Definite Proportions,

and if Englishmen seriously propose to themselves the task, we are justified by the history of Science in asserting that none are more likely than they to solve the great problem of mineralogy which now offers itself—the connection of chemical composition and crystalline form." Such were the words of Dr. Whewell in 1832.

Indeed, we have only to recall the additional names of Wollaston, Brewster, and Miller to remind ourselves that much of the most original work of the century has been done by English mineralogists. An appreciative reference, in Mr. Fletcher's address, to the highly philosophical teaching of Professor Maskelyne on crystalline symmetry, shows that the true spirit is by no means dead among us even in the most abstruse branch of the science; and Mr. Barlow has recently made important contributions to the same subject. Professor Renard, of Gand, in seconding a vote of thanks, declared that no one had done more to elevate the respect already felt abroad for British mineralogy than the distinguished President himself, and referred especially to his admirable "Introduction to the Study of Minerals," which was published in the form of a guide to the vast collection, of which he has the charge, at South Kensington.

ANTHROPOLOGY.

Sir William Flower performed a most useful task when, in his presidential address, he reviewed the past history and the future prospects of anthropology. As a separate branch of science anthropology is so new, and on many of its sides is so largely aided and furthered by the amateur, that it was well worth while pointing out that the scientific training and equipment for the scientific study of anthropology is as great as those required in any of the older sciences. "It invokes the aid of the sciences of zoology, comparative anatomy, and physiology, in its attempts to estimate the distinctions and the resemblances between man and his nearest allies, and in fixing his place in the scale of living beings. In endeavouring to investigate the origin and antiquity of man, geology must lend its assistance to determine the comparative ages of the strata in which the evidences of his existence are found, and researches into his early history soon trench upon totally different branches of knowledge. In tracing the progress of the race from its most primitive condition, the characteristics of its physical structure and relations with the lower animals are soon left behind, and it is upon evidence of a kind peculiar to the human species, and by which man is so pre-eminently distinguished from all other living beings, that our conclusions mainly rest. The study of the works of our earliest known forefathers—'prehistoric archæology,' as it is commonly called—is now almost a science by itself."

Towards the conclusion of his address the President gave an account of the conclusions arrived at by the committee appointed by Mr. Asquith to inquire and report upon the "anthropometric"

methods of registering criminals. He expressed complete agreement with their recommendation of a modification of the French system of measurements, with the addition of Galton's "finger-print" method.

The Antiquity of Man.

It is interesting to watch the gradual change of opinion as to the antiquity of the human race, and to compare what is said at Oxford in 1894 with discussions of the same question at former meetings of the British Association. At one time the debate was largely one for theologians to take part in, and personalities were freely used; now a joint meeting of the anthropologists and the geologists to discuss the Plateau Flint Implements of North Kent leads to a thoroughly scientific and calm review of the whole question. It was unfortunate that age and illness should have prevented the Nestor of our geologists, Professor Prestwich, from taking part in so interesting a debate, and it was unfortunate also that Mr. W. J. L. Abbott, who has done so much good work in the Plateau Gravels of recent years, and whose interesting paper on this very subject will be remembered by our readers, should also have been too unwell to attend; otherwise nearly all the leading authorities took part in the debate.

Professor T. R. Jones opened the discussion on behalf of Professor Prestwich, maintaining his well-known views as to the vast antiquity of the rude implements found on the higher plateaus. Mr. Whitaker doubted whether many of the supposed implements exhibited at the meeting were really of artificial origin, but acknowledged that some were certainly genuine. Most of these had been picked up on the surface, not dug out of undisturbed gravels. He questioned whether the discovery of implements in the "clay with flints" was any evidence of age, for the clay with flints was still in process of formation. Mr. Montgomerie Bell thought that he could trace a continuously improving series of implements in the different deposits of Kent, but Sir John Evans doubted whether Kent was really the cradle of the human race.

Dr. Hicks spoke of the relation of the implement-bearing gravels of Middlesex to the glacial and cave-deposits of North Wales, and insisted that man was pre-Glacial. Professor Boyd Dawkins pointed out that rude workmanship was no satisfactory evidence of antiquity, as rough or finished implements would be used according to the purpose for which they were required. General Pitt-Rivers spoke on the gradual evolution of implements of different types. He did not consider the mere occurrence of a bulb of percussion as sufficient evidence of artificial origin, and doubted the value of the evidence as to the implements having been found in place in the plateau deposits. Mr. Clement Reid questioned the extreme antiquity of the implements, pointing out that careful search had led to no discoveries in undoubted Pliocene and pre-Glacial deposits. Colonel

Godwin-Austen suggested that much of the apparently artificial chipping of the flints had originated through the rubbing together of stones in masses of talus and mud. On the whole, then, the general sense of the meeting was opposed to granting any extreme antiquity to the early dwellers on the former plateau of Kent.

FORESTRY.

Professor Bayley Balfour, in his capacity as President, addressed the Biological Section on the subject of Forestry. In many respects this address was the most important communication that was delivered to the public in any of the Sections of the Association.

Two reasons have interfered to prevent the English people paying necessary attention to the afforestation of our land. The first is the abundance of coal in this country. For many years all practical men have regarded the supply as almost inexhaustible, and it is only now that the warnings of experts in mining and geology have been accentuated by the actual exhaustion of many of our coal-fields that there is a possibility of getting the public to believe that England may soon have to revert to the partial use of charcoal. The next reason is that the continuous extension of our commerce and our territory has opened up for use series of forest-clad areas, while the woods and forests of the continent have by themselves been nearly able to supply our demands. But the increasing scarcity and price of imported timber is now awakening our eyes to the enormous waste of our timber-growing capacity. This waste is in two directions: the forests we have, are, from want of trained experts, in many cases very badly managed; immense areas, well adapted for growing timber, and ill adapted for any other crops, are left barren.

Besides the direct economic advantage of increasing the productiveness of the land, there are many other advantages that would accrue were the course urged by Professor Bayley Balfour and others to be adopted. There would be a great increase in the employment of labour; a very large part of at least the first steps in afforestation would demand unskilled manual labour, directed by a comparatively small staff of experts. We believe that the London *Daily Chronicle* has already advocated this as a field for that State employment of the unemployed which is believed by many to be at once prudent and inevitable.

The extension of woodland and forest would increase not only the natural amenity of our country, but it would appeal directly to all naturalists and lovers of animals and plants, in that it would provide an enormous increase of area suitable for many of the most attractive wild animals and plants that can live in this country. Professor Bayley Balfour made a practical suggestion to aid the attainment of his object. "There are tracts which without damage to the natural beauty, and without depriving in any sensible degree the people of their privileges of recreation they prize so much, might

be and should be dealt with as forest cultivated on scientific principles. These might serve as instruction areas, showing all that is best for the information of foresters. The creation of some such experimental teaching stations in State forests is one of the essentials for forestry in Britain. I would go further, and say that the area of State ownership should be increased to the extent of the establishment of forest stations, of an acreage sufficient to allow of a satisfactory rotation, in other parts of the country as centres of instruction." "In a practical science like forestry an increase of sound technical knowledge can only be possible when facilities for practical instruction are provided. I would, therefore, ask the President of the Board of Agriculture to consider what I have just said with regard to State forest experimental areas. These cannot, of course, be created by a stroke of the pen, but the initiative for their formation would naturally come from the Board of Agriculture." "It appears to me that while we must obtain from the Government the institution of sylvicultural areas for practical instruction, our best chance of success in acquiring the necessary endowment for the rest of the teaching lies in the line of combination between the Board of Agriculture and the County Councils, with, it may be, aid from private benefactors. But if we were to draw financial support from County Councils or from private sources, we must as a first step towards this make known, more thoroughly than it is, the nature of the national interests involved. We must disabuse landowners, land agents, and practical foresters of the notion that forestry consists in the random sticking in of trees, which anyone, no matter how unskilled, may accomplish. We must bring home to the people's minds that in science is to be found the only sure guide to proper timber growing, and that scientifically managed forests are alike a profit to the producer, a benefit to the community of the region in which they are reared, and a source of national wealth. Once we have got so far as to create this opinion, the funds for as extended a scheme of forestry education as may be necessary will, I venture to think, be forthcoming."

Turning from the duties of the nation to the duties of botanists, Professor Balfour urged upon them the necessity of paying more attention to the subject, for it is from the ranks of botanists that the directors of future schemes of afforestation and the teachers of practical foresters must come.

The Nucleus in Plants.

The next subject of botanical interest we shall mention is a very technical one, and a subject in which none but the scientific public could possibly take an interest. At one of the meetings of the Biological Section Professor Strasburger gave an admirably clear account of his researches on the periodic variation in the number of chromosomes in the nuclei of plant cells. After discussing the

probable history of sexual and asexual reproduction in plants, Professor Strasburger went on to speak of plants which exhibit alternation of generations. In these forms it is invariably found that in the nuclei of the germ-cells of the asexually produced individuals, a definite number of chromosomes is always present when the nucleus is in an active stage. This number appears, at any rate in the case of the lower cryptogams, to be twelve. Hence it follows that a fertilised ovum contains twenty-four. This latter number characterises every cell in the body of the sexually produced individual up to a certain point, *i.e.*, the appearance of the egg or spore mother-cells. At this point a reducing division sets in, and the number of chromosomes in every spore formed is lessened to half its original number *i.e.*, twelve.

The exact nature of this reducing division is not yet evident.

Carrying these observations further, he finds that in the higher phanerogams, where the sexual generation may be suppressed, the same sudden reduction from sixteen to eight chromosomes occurs previously to the formation of the germ-cells. From these facts he concludes that the reducing division in these plants marks phylogenetically a more primitive condition in which alternation of generations obtained. Professor Bower is translating Professor Strasburger's paper, and the full text of the translation will appear in a forthcoming number of the *Annals of Botany*.

PHYSIOLOGY.

Professor E. A. Schaefer's presidential address derived additional interest from the circumstance that this is the first year since 1847 that there has been a separate section for physiology. While pointing out that the enormous increase in England of those devoting themselves to physiology as a special science had led to the reappearance of this as a separate section of the Association, Dr. Schaefer suggested that it might not be advantageous to have one at every meeting of the Association. "Physiology is, above all things, a practical science. It requires laboratories and means of demonstration. Physiologists are rarely satisfied with the opportunity of hearing and reading papers; but require that, as much as possible, the actual methods of research employed should be capable of demonstration. By this I am not to be supposed to advocate the demonstration of experiments upon animals, for there are very many subjects in physiology which can be both worked at and illustrated in a manner involving in no sense whatever the word vivisection."

We differ from Professor Schaefer's conclusion. To very few of the sections would his words about "practical science" and so forth not be applicable, and for the purposes of demonstration and explanation of the methods of research, there are special gatherings of specialists. In the case of physiology, there is that very energetic Physiological Society, to which Professor Schaefer referred, and

which meets in the physiological laboratories of its members for the precise purposes to which Professor Schaefer alluded. There are subjects galore on which papers can be read and discussions raised without the necessity of laboratory demonstrations. In many respects, however, we cannot help regretting the breaking up of the section of biology into separate sub-sections and sections. It tends more and more to stamp the Association with that isolated specialism which is inevitable in modern science, but which carries with it great disadvantages. If the zoologists meet by themselves in a zoological laboratory, the physiologists in a physiological, and the botanists in a botanical, the odds are that the subjects they discuss will be of a nature to be unintelligible or uninteresting to those of another specialism. We imagine that a botanist or a zoologist would gather little from those demonstrations of the methods of research in a physiological laboratory which Dr. Schaefer advocated. What we want at the meetings of the British Association is an increase of organised discussions of subjects which lie on the borderland of the different branches, and of the different branches of these branches.

To take an example, one of the subjects of Dr. Schaefer's address was recent discoveries about the cell, and specially about the nucleus and the centrosome. Dealing with the researches upon the centrosome of the physiologist Heidenhain, the President described it as one of the obscure subjects of which, though supremely important, physiology knew next to nothing. Now, in the Biological Section, Professor Strasburger read a paper on the nucleus and centrosome in plants, while Dr. Van Beneden discoursed on the same topic from the point of view of a zoologist. Is not this a lamentable instance of failure to take advantage of the opportunity of joint discussion? Very many similar subjects might be mentioned. For instance, at the present time there are different and novel views of the nature of heredity and of the structure of protoplasm propounded by botanists, physiologists, and zoologists. And elsewhere in this article we allude to similar lack of concert on subjects of Geographical Distribution and Darwinian Theory. We are certain that well organised discussions of these held by biologists from all the sub-sections of biology would prove not only of surpassing interest but of great value. Although the physiologists do not intend to meet as an independent section next year, yet the botanists promise to carry on the evil.

Natural Selection.

The most interesting discussion in the Biological Section was confined almost entirely to zoologists. Professor D'Arcy Thompson boldly threw down his gauntlet by stating his doubts and difficulties in accepting "Darwinism." He could not see how many forms and colours of animal life were to be explained on the simple hypothesis of Natural Selection, even assisted by Sexual Selection; and he instanced the brilliant colours of humming-birds and the logarithmic

spiral of a coiled shell, whether foraminiferal or molluscan, as due rather to physical laws governing growth than to any advantage which they might give their possessors in the struggle for existence. The ingenious speculation that the pear shape of a guillimot's egg is derived from the protection that it affords to the egg from falling off the rock ledge, did not find favour with Mr. Thompson, who pointed out that the same shape was found in eggs, such as the plover's, laid on the broad flat ground. This shape he ascribed to the mechanical pressure exerted by a narrow oviduct on a relatively large egg.

Professor C. V. Riley, the eminent entomologist of Washington, was prevented by the strict time-limits from properly presenting his suggestive conclusions derived from a study of social insects. This we hope partially to remedy by publishing his valuable paper *in extenso*. Broadly put, his opinion was that Natural Selection was an insufficient or impracticable cause in the evolution of the varied groups of individuals met with in an insect colony, and that much was due to the direct action of environment, notably of food. These are views to which much attention has already been directed in our own columns.

Professor J. Berry Haycraft, who felt constrained to describe himself as a physiologist, but not a biologist, raised a most interesting objection to the views held by Weismann as to the part played in evolution by sex. He pointed out that the conjunction of two parents, instead of promoting variation, restrained it and kept it near the mean. This, he said, was proved by the statistics collected by Francis Galton; without sex, we should have no genera or no species, only individuals. This interesting communication will appear in a future number of NATURAL SCIENCE.

Dr. F. A. Dixey contributed to the discussion a detailed account of the evolution of mimetic characters in certain butterflies, without, however, very clearly showing how the gradual stages that led up to the completely mimicking forms could have been due to any struggle for existence or any protective advantage.

Professor H. F. Osborn, of Columbia College, New York, entitled his paper, "Certain Principles of Progressively Adaptive Variation observed in Fossil Series." A large part of it, however, was devoted to emphasising the necessity for analysing variations according to their character and the period of ontogeny at which they occurred. Thus, he divided all variations into Palæogenic and Neogenic, the former being instances of reversion, and therefore not entering, like the latter, into progressive evolution. Neogenic variations may again be divided into four classes:—(1) Gonogenic, due to variation in the original ovum or spermatozoon before their union; (2) Gamogenic, due to the union of diverse sexual elements; (3) Embryogenic, due to the influence of environment on the developing embryo; (4) Somatogenic, due to the action of habits or changed environment on post-

embryonic stages. Mr. Osborn pointed out that a study of the development of variations through a long lapse of time showed that similar structures might be reached by quite different roads, as instanced by the molar and pre-molar teeth of *Anchitherium*. He seemed inclined to attribute this more largely to direct mechanical stimulus acting towards the same given end, than to the action of Natural Selection.

After these various onslaughts, the discussion promised to be lively. But Professor Ray Lankester and Professor Poulton, who were the chief supporters of Natural Selection, dealt with the obvious misapprehensions of Darwinism in the papers of those who spoke against Natural Selection rather than with the details of their objections. While we have the strongest sympathy with those who point out the astonishing misconceptions of Darwin's own views, which, unfortunately, are so prevalent with those who bring forward difficulties in the theory of Natural Selection, we think that the attitude adopted by the supporters of Darwinism is unfortunate. For the general public are even more ignorant of Darwin's own writings than those who urge scientific objections to his theories. The impression left on the minds of the camp-followers of science is, that the defenders of Natural Selection are more concerned to maintain an orthodox interpretation of the Darwinian scripture than to do battle about the actual facts.

Mr. Pycraft followed this discussion with a valuable account of the wing of *Archæopteryx*, which gave rise to an animated discussion, in which Dr. Sclater, Professor Ray Lankester, Dr. Hurst, Mr. Finn, and others took part. As, however, this paper will shortly appear in our pages, further description is now unnecessary.

The Distribution of Animals.

This subject, in one or other of its varied phases, was dealt with in many of the Sections, and it seems a pity that some arrangement could not have been made by the organising committees to enable those interested to meet and discuss the question at the same place and time. Professor W. A. Herdman endeavoured to accommodate himself to the situation by reading his valuable Report on the Marine Zoology of the Irish Sea before the Geological as well as the Zoological Section. The result, however, was that in neither place could it receive the attention which all felt that it deserved. The Committee, of which Professor Herdman is both Chairman and Reporter, has paid particular attention to the deposits now forming on the floor of the Irish Sea. It has mapped out the distribution of the various sands, muds, shell-conglomerates, clays, and nullipore deposits, and has endeavoured to see how they are connected with the older geological formations of the sea coast and the submarine deposits lying off the shore. It has further come to the very important conclusion that the nature of the deposit is the most important of the

various factors that determine the distribution of animals over the sea-bottom within one zoological area. It is proved to be more important than mere depth; while the same locality may vary so much from time to time in the temperature, the salinity and the transparency of the water, that these factors, within certain limits, have far less influence upon the fauna than has the nature of the deposit. In the discussion that took place on this paper, Mr. F. A. Bather pointed out how important the study of these modern conditions was to the geologist who attempted to correlate strata by means of their fossil contents; and he suggested the possibility that at some future epoch, when the floor of the Irish Sea had become dry land, these absolutely contemporary deposits might come to be regarded as a series of successive formations, each characterised by its peculiar "zonal" fossils. A similar state of affairs in the Silurian rocks of Gotland has actually given rise to much heated controversy.

Among the more interesting discussions in the Geological Section was that which arose out of Dr. Hicks' paper on The Homes and Migrations of the Earliest known Forms of Animal Life as indicated by Recent Researches. Few of the speakers, however, seemed to confine themselves to the real questions at issue, a fact which may have been due to a want of clear understanding of Dr. Hicks' position. We hope that our readers will be able soon to form their own opinions on the subject, as this interesting communication will appear in a future number of NATURAL SCIENCE.

In the Zoological Sub-section Dr. Otto Maas, whose paper on "Some Problems of the Distribution of Marine Animals" (NATURAL SCIENCE, vol. ii., p. 92) our readers will remember, gave an interesting account of "temperature as a factor in the distribution of marine animals." It was pointed out by Messrs. H. N. Dickson and W. Garstang, during the discussion, that much variation of fauna, apparently due to variation of temperature, might really be due to an influx of water from other sources and to the consequent introduction of foreign forms of life, or the change of the chemical and physical characters of the water. Most speakers, however, seemed to think that so important a paper should be published before it could be discussed. We have much pleasure in announcing that the complete work will appear in an early number of NATURAL SCIENCE.

These seasonal movements of the water in the North Sea and in the Atlantic, which have so much bearing on this subject, were discussed more fully by Mr. Dickson before the Geographical Section. Here also there was a discussion on the distribution of African fish, and, finally, it was here that Dr. John Murray thought fit to read his important paper on the Geographical and Bathymetrical Distribution of Marine Organisms. In the course of this he attempted to demolish the Antarctic Continent of Mr. H. O. Forbes, a question on which geologists and zoologists, no less than geographers, might well have been asked for their opinion.

Maxim's Flying Machine.

Although artificial flying is a problem to be settled by engineers rather than by zoologists, it is a subject which always must have a special fascination for zoologists. It was therefore natural that a number of zoologists should have deserted to the mechanical section to hear Mr. Maxim.

The subject of his paper is of fascinating interest, and the accounts of his experiments, which have extended over some years, were also exceedingly interesting. As might be expected from a man of Mr. Maxim's inventive genius, something has come out. The first is a steam engine of hitherto unrivalled lightness compared to the enormous power it can develop, and a system of aeroplanes which, as the author claims, has enabled him to be the first to fly—not for long, it is true, but that is, after all, only a question of time. The question of landing safely has not yet been solved, and there seems to be some difficulty, as the machine ceases to maintain itself at anything under thirty-five miles an hour.

While Mr. Maxim has not yet got a perfect flying machine, he certainly has advanced considerably towards the solution of the problem; though it may be questioned whether he has, in using the system of aeroplanes, hit on the best plan, that is if one of the conditions is that the flying machine has to be used for warlike purposes, and not for rapid transit. Perhaps something of the kind suggested by Lord Kelvin is more likely to effect this object.

Lord Kelvin and Lord Rayleigh had both ridden on the machine, and expressed themselves highly pleased with the extremely skilful way in which Mr. Maxim had overcome the various difficulties the condition of lightness in everything had imposed upon him. Mr. Langley, a notice of whose recent publication, on "The Internal Work of the Wind," appears on our next page, sounded a note of warning about the liability of the machine to turn, which, coming from such an authority, must not be disregarded.

Many other points of interest were raised at the meeting, which we have not space to discuss. We hope to recur to some of them on a future occasion.

SOME NEW BOOKS.

THE INTERNAL WORK OF THE WIND. By S. P. Langley. Published by the Smithsonian Institution, Washington, 1893.

To understand how birds fly, and to imitate them ourselves, were primitive ambitions of the race; and ambitions, in spite of Mr. Maxim's aeroplanes, they remain. By severe exertion Lilienthal was able, for a few moments at a time, to develop an ascensional force equal to something less than his own weight. The condor and other large birds poise themselves for hours in mid-air, rising, falling, and progressing with no apparent effort, and without the slightest vibratory movement of their expanded wings. The omission of some important factor in the theory can alone account for so great a discrepancy in the result. That this factor is in some way *the wind* has been long suspected; but the idea, sometimes held, that a uniform current (however strong) can for long assist the soarer, is at variance with the elementary principles of dynamics. In order that the bird may rise like a paper kite there must needs be something to take the place of the string. In a uniform current there is no force available for the purpose except the bird's inertia during the first few seconds after leaving the ground; but that birds do use their inertia, at all events in this limited way, is proved by the observation that many of them are unable to soar without facing to windward at starting. If, however, the current is *not* uniform, the limitation disappears, for in passing from a quicker to a slower current, or *vice versâ*, inertia again comes into play. The question, therefore, remains, do such inequalities exist, and if so, are they adequate to account for the phenomena? Professor Langley's paper answers both parts of the question affirmatively, and it is not too much to say that his experiments constitute by much the most important advance yet made in the theory of aërial flight. "In the ordinary use of the anemometer," he writes, "the registry is seldom taken as often as once a minute." His first experiment was to place an ordinary Robinson anemometer on a mast fifty-three feet high and connect it electrically with the chronograph of the Allegheny Observatory, so as to register every 25 revolutions of the cups. The results are drawn out in a diagram with abscissæ = Time, and ordinates = Wind velocity. In a period of about five minutes, taken at random from the diagram, the wind velocity is seen to have fallen from 13 miles an hour to 9, risen to 17, fallen to 14½, risen to 15, fallen to 14, risen to 19, fallen to 13, risen to 18½, fallen to 14, and risen to 17. This degree of variability in "the wind" is remarkable enough, but Professor Langley's research did not stop here. He proceeded to lighten the cups and to make an automatic record of *every revolution*. The resulting curve often exhibiting nine or ten cusps within a single minute, and deviations of 30 or 40 per cent. on both sides of the line of mean velocity, reveals

a state of things hitherto quite unsuspected either by meteorologists or theorists on flight. Extreme variability seems to be equally characteristic of strong and of gentle winds, nor is there any reason to suppose that a still lighter instrument recording still more frequently would not bring to light further inequality. It is quite likely that "the incessant alterations which, it appears, make 'wind,' are due to impulses and changes which are preserved in it, and which die away with considerable slowness." What we should like to know more of is the change (if any) in *direction* which accompanies the change in velocity. Is the "structure" of the wind mainly undulatory or mainly vertical? that is to say, are the variations observed at a fixed point due to the passage of "ripples" or minute "cyclones"? Upon the answer to this question, it seems as if the future of what Professor Langley calls "Aërodromics" will largely depend.

R. C. GILSON.

PLANT CLASSIFICATION.

UEBERSICHT DES NATÜRLICHEN SYSTEMS DER PFLANZEN. Zum Gebrauch in Vorlesungen für Anfänger. By E. Pfitzer. 8vo. Pp. iv., 36. Heidelberg, 1894; Price 1s.

PROFESSOR PFITZER'S "Übersicht des Natürlichen Systems der Pflanzen," designed primarily for students attending his course of lectures for beginners, will, we doubt not, be found useful at centres of learning other than Heidelberg. It is a small octavo volume of thirty-six pages, with one side blank for notes, and contains a sketch of plant classification. In a few lines are given the chief characteristics of the great groups and divisions, and, *à propos* of these, we cannot fail to notice the changes which have occurred in this branch of the science during the last few years. Those of nomenclature, though necessitating the loss of well-known terms, indicate an advance, in that real and not imaginary differences are now expressed. Instead of the Phanerogams and Cryptogams of the text-books of not many years ago, we now find plants ranged under the three great groups, Siphonogamæ (equals Phanerogams), where the egg, or oosphere, is contained in an ovule and fertilised by the agency of a pollen-tube; Archegoniatæ, where the egg is contained in an *archegonium* and fertilised by an independent cell—the *antherozoid*, and including (1) the ferns and their allies, the lycopods and equisetums, (2) the mosses and liverworts; and, thirdly, the Thallophyta, plants which form neither archegonium, ovule, nor pollen-grain, and including the two great divisions of Algæ and Fungi.

Of the two divisions of the Siphonogamæ, Angiospermæ and Gymnospermæ, the latter, in which, though fertilisation takes place by means of a pollen-tube, the egg is contained in a rudimentary archegonium, finds a most natural position at the end next the true Archegoniatæ. We may also note the tentative subdivision of the Angiosperms into Acrogamæ where the pollen-tube reaches the egg by means of the micropyle, and endosperm is formed after fertilisation, and the Chalazogamæ, where the mode of entrance is by way of the chalaza, while endosperm is formed before fertilisation. The former include everything except the Australian genus *Casuarina*, which constitutes the order Casuarinaceæ, and at present the subdivision Chalazogams. In the subdivision of the dicotyledons with free petals (Choripetalæ) there has been much alteration from Bentham and Hooker's separation into Thalamifloræ, Disciflorae, and Calycifloræ. Only the last of these remains as one of six, while the whole is enlarged

by the intercalation of the families formerly separated as the anomalous division Apetalæ. Some of these, like the Chenopodiaceæ and Amarantaceæ, fall naturally into their places, but the affinity of others, like Salicaceæ (the willows) is doubtful or quite unknown, while others again comprise distinct series. Under each natural order (the very rare or small ones are omitted) the most important characteristics are given, with the floral formula and the broad geographical distribution. In the case of larger orders, division into tribes is noticed, in the smaller only the endemic genera and the species of economic importance.

Professor Packard on Acquired Characters.

On the Inheritance of Acquired Characters in Animals with a Complete Metamorphosis. By Alpheus S. Packard, M.D. Reprint from the *Proceedings of the American Academy of Arts and Sciences*, 1894, pp. 331–370. Boston, Mass.

The historical *précis* with which Professor Packard opens his paper very naturally consists largely of an exposition and criticism of Weismannism. In this he makes one serious error. By quotations from the "germ-plasm," he supports his statement that "Weismann's reasons for not accepting the doctrine of transmission of acquired characters would appear to be purely hypothetical and *à priori*." It is certainly true that Weismann has adduced purely theoretical and *à priori* reasons. But again and again in published papers he has examined supposed cases of the transmission of acquired characters and of mutilations, and has shown the considerations of actual fact which support his hypothetical and *à priori* objections. As a matter of history, each term in the continuous adduction of fresh cases, or supposed cases, of such a transmission, has been brought forward after Professor Weismann and his supporters have shown reasons for not accepting preceding terms. The present position of the controversy is this. As yet no case of the transmission of acquired characters has been brought forward based on evidence accepted as cogent by Professor Weismann and his supporters. Professor Packard may say that this is so much the worse for the intelligence of Professor Weismann and his supporters. He may not say that their reasons are purely hypothetical and *à priori*.

Professor Packard reviews a number of the arguments brought forward by the advocates of this transmission and adds several of his own. We are, however, compelled to state that the arguments are not presented so as to convince any but the already convinced. Take one of his instances. He cites Paul Bert's experiments upon daphnids, in which he attempted to acclimatise these fresh-water crustacea to saline water by gradually adding salt. At the end of forty-five days, when the water of the aquarium contained 1·5 per cent. of salt, "all the Daphniæ had died; but the eggs contained in their brood-sacs had survived, and the new generation of Daphniæ, to which they had given birth, flourished perfectly well in the same medium." "We should interpret these facts," says the Professor, "as showing that the crustacean had been so profoundly affected in the lifetime of the individual as to produce young perfectly adapted to a changed environment. The germ-plasm may have been the vehicle, all the same, but the experiment is a case in favour of the neo-Lamarckian principle." Was ever argument less cogent? Here are animals supposed to transmit to their offspring a character before they have acquired it: to transmit to their eggs a capacity to endure salinity, to the non-acquisition of which they testify by their own deaths.

Moreover, the offspring, with respect to this character, are in a position in which it is impossible to judge of what, in their case, is inherited and what is acquired, for they are subjected to the new influence from their earliest birth. As in this instance, so in many others, Professor Packard walks round the point at issue. All instances of the power of environment to mould the individual are of the greatest value; and Professor Packard and others have done service in directing attention to a side of nature perhaps neglected. But, for this particular controversy, what is wanted is proof of the inheritance of the result of the moulding.

The actual occasion of this publication is a careful and valuable account of the complicated metamorphoses of many insects, accompanied by suggestions of causes in the environment which might have produced the characters of the stages in question. Students of evolution and of natural history will find this valuable, but we submit that none of it has a direct bearing upon the controversial problem placed as the title of the paper. In every case the argument goes no further than to suggest that, if acquired characters be transmitted, and if the Lamarckian factor in modification of organisms have the value assigned to it by Professor Packard, then the characters in these stages of larval history might be explained as due to the inheritance of acquired modifications. But all this, although it has a place, and a useful place, in scientific writing, takes us no further towards a solution of the problems in question, and gives support to the uneasy suggestion made at the recent discussion at the British Association that many of the opponents of Natural Selection do not understand the nature of the problems about which they write.

Zoology for Farmers.

Agricultural Zoology. By Dr. J. Ritzema Bos, Lecturer in the Royal Agricultural College Wageningen, Holland. With an Introduction by Miss E. A. Ormerod; Translated by J. R. Ainsworth Davis, B.A., Professor in the University of Wales. Pp. 256 and xx., with 149 illustrations. London: Chapman & Hall, 1894. Price 6s.

Dr. Ritzema Bos is so well known as a competent scientific naturalist who has specialised on what is called economic zoology, that we welcome an English rendering of his little book. It is designed to be a handy book for the practical farmer, and it deals with the animal kingdom in such a way that the private student shall have an intelligent idea of the general structure and organisation, and of the place in the scale of life of those animals that are harmful or helpful to agriculture. Reference to the domesticated farm animals is omitted, "for these are treated not by the zoologist but by the lecturer in stock-breeding." The translator, in his preface, says that he has made a few small additions and alterations, to adapt the book for those who live in England. Miss Ormerod, in her introduction, gives the book her blessing, and commends it to agriculturists.

The English is easy and intelligible. The illustrations, especially those dealing with insects and eelworms, are well adapted for their purpose. So far as the make-up of the book goes, we have praise for everything but for the lamentable absence of an index. The book ought to go into other editions. We hope that the technical instruction committees of County Councils will make large use of it for their teachers and for their students, but in another edition an index must be added—an index containing the common English names of all the animals referred to.

Dr. Ritzema Bos is a kindly man, and is ready to say a good word for wild animals as often as he can. Among mammals and birds he sets against each other the good and the harmful influences upon agriculture, and condemns unhesitatingly very seldom. Thus foxhunters will be pleased to learn that the fox "is perhaps generally of more use than otherwise to the farmer and the forester. It catches many rabbits, and also an enormous number of field voles, in the years when these become a pest. It also often eats insects (*e.g.*, cockchafers), worms, and snails." The stoat is decided to have a balance of good qualities, the weasel is almost uncompromisingly praised for its usefulness as a vermin destroyer; the common shrew, the lesser shrew, the hedgehog and the bats are all to be preserved. On the other hand, for the water shrew, the rats and mice, and the voles, Dr. Bos suggests a careful preservation of their natural enemies, and a bill of fare of which phosphorous paste, dressed in various attractive fashions, is the principal ingredient.

We are less satisfied with his treatment of birds. It is true that he advocates the preservation of ali the British owls and of the kestrel and the buzzard. But he says that "predominantly harmful from killing domestic mammals" are the following species occurring in Britain: The sea eagle, the golden eagle, the peregrine falcon, the merlin, the hobby, the sparrow hawk, the goshawk, and the harriers. The result of the examination of stomachs proves that in many of these cases a large balance of good is done by the birds. As for the other cases, though it be true that *aquila non capit muscas*, the eagle destroys no insect pests, we have no sympathy for the naturalist who plays the informer. It is a poor consolation to know that few English farmers are likely to be driven by depredations on their flocks to set springes to catch golden eagles, or to go out with shot guns against sea eagles!

Among other birds the black list is less offensive to us, although we fear that the naturalist and the agriculturist will never quite see eye to eye in the matter of the destruction of any kind of birds. It includes the raven, the magpie, sparrows, and linnets. Because of its shortness it marks so great an advance on popular notions that we will pass it by in silence.

A good and much needed word is spoken for the persecuted toad. In addition to its attacks upon insects, the toad is a great destroyer of snails. "In the Research Garden attached to the Rouen Entomological Laboratory the snails were entirely exterminated in 1891 as a result of introducing 100 toads and 90 frogs."

Although we have occupied so much of the space at our disposal in discussing the part of Dr. Ritzema Bos' book that deals with Vertebrata, as a matter of fact the greater part of the book itself deals with injurious insects and worms. For all this we have nothing but praise. It would be of inestimable advantage to farmers and to gardeners were they to have an acquaintance with the facts about these common pests which are so clearly explained in this excellent treatise.

MONISM.

THE MONIST. A Quarterly Magazine. Vol. iv., no. 4. Chicago: The Open Court Publishing Co. London: Watts & Co. Price 2s. 6d. July, 1894.

WE fancy that monism, as yet, subtends a small angle in the minds of most of our readers; we may, therefore, take advantage of an article in the current issue of the *Monist*, written by the editor of the *Monist*, and

entitled "The Message of Monism to the World," in order to give a very brief account of the doctrine. Until a few years ago, in the minds of most people who reflected upon the meaning of things, and in the minds of each of the great mass of men who accepted with easy-going indifference what was thrust upon their consciousness, mental images of the universe were broken up among a series of separate identities. Thus, in the sphere of theology, there were the Creator and the things created; in psychology, there was the central ego, and the surrounding swirl of sensations and states of consciousness, there were mind and body acting and reacting upon each other. In science there was matter, and force acting on matter, while even to different kinds of matter and varieties of force there were ascribed separate existences.

In all these the attitude of the thinking world is slowly changing. Formerly, man attempted to express his sense of the dependence of all things upon a Supreme Being in the phrase "Omnipresence of the Deity"; now we attempt to express the same relations in the phrase "Immanence of the Deity." The change of phrase marks an attempt to destroy the sense of separation between the Deity and the universe, or, rather, to destroy the idea of the co-existence of two separate identities. Similarly, in psychology there is a tendency to be rid of a separate abstract metaphysical "ego," to merge the abstraction in the sum of states of consciousness. There is not an "I" which has sensations, emotions, and ideas; the sensations, emotions, and ideas are part of the "I." The change is better known in things scientific. When now we speak of matter and force acting and reacting upon each other, we are ready to admit that matter in the last analysis resolves itself into the action of forces, and that we know force only through its action on what we call matter. In the separate sciences the change of tendency is still plainer. We seek to interpret the elements in terms of each other, perhaps as unsuccessfully as Lord Salisbury seems to think, but with a hope greater than his, and we believe in the conservation of energy and in the transformation of chemical, electrical, physical, and perhaps even vital forces.

"Monism" is the pointed, progressive, and polemical expression of all this change of attitude. It is, to use the words of Dr. Paul Carus, "the new philosophy that is dawning upon mankind: the theory of oneness, which indicates that the world, we ourselves included, must be conceived as one great whole. All generalisations, such as matter, mind, and motion, are abstractions representing aspects of reality, but not entities or things-in-themselves, by a combination of which the universe has been pieced together. And all our notions of nature can be formulated in exact statements, which, when properly understood, form one harmonious system of natural laws." No doubt, like the active and aggressive form of every idea, it has coated itself with a certain brusque and repellent dogmatism; but soldiers actually on campaign are not beautiful objects: fine feathers, necessarily, are reserved for parade. But anyone who studies it may see that it is an honest attempt to combat the arrogant narrowness of agnostic and religious dogmatism.

Alaska.

Those interested in Alaska will find in the August number of the *Scottish Geographical Magazine* an epitome of the knowledge we possess of that little-known land, from the pen of Israel C. Russell. Mr.

Russell has made three journeys into the country himself, and he completes his account from the narratives of others who have travelled on various business in the country.

It is difficult at first to realise that Alaska is eighteen times the size of Scotland, and that it has a general coast-line of 4,000 miles, or, if the shores of the bays and islands be measured, of 11,000—12,000 miles. The river Yukon divides the country in two approximately equal portions. It rises in the north-western part of Canada, flows westward across the territory, and falls into the Behring Sea. The Yukon has a length of 2,000 miles, and drains an area equal to 440,000 square miles. It has every appearance of great age, and has been repeatedly disturbed and obstructed by volcanic action. The vast mountain system follows the coast, and is a continuation of the Cordilleras. The culminating peaks of the range are found in Alaska, and are Mount Logan, 19,500 feet, and Mount St. Elias, 18,010 feet. A host of other peaks, several of which exceed 14,000 feet in height, make this region one of the most rugged and impassable in the world.

The region is one of volcanoes, and eruptions of more or less magnitude are of frequent occurrence. Mr. Russell notes that Shishaldin, in the island of Unimak, is a symmetrical cone with gracefully curving sides of the same type as Fuji-San, and rises to the height of about 8,000 ft. The wreath of smoke proceeding from the crater is a well-known beacon to mariners. On the shores of Behring Sea and of the Arctic Ocean the lofty mountains disappear and give place to the "tundras" or low, nearly-level, moss-covered plains of 70—100 miles broad. During the summer these tundras are swampy and covered with mosses, lichens, small flowering plants, rushes, and ferns. The most conspicuous plants are the dwarf willows, which attain a height of about 2 ft. The soil is a black humus, and at a general depth of a foot or thereabouts is always frozen. This frozen subsoil is not confined to the tundras, but in many places along the Yukon river the banks are formed of horizontal sheets of ice many feet thick, covered with moss, and supporting a dense forest of spruce. In hot summer, when the temperature is from 90°—100° F. in the shade, one may brush away the moss at his feet and find solid ice beneath.

In some places a depth of 300 feet of black, peaty humus has been observed, and Mr. Russell notes the interest of these deposits in connection with the formation of coal.

The glaciers of Alaska are, with the exception of those of Greenland, the largest and most instructive in the Northern Hemisphere. The largest is the Malaspina, on the south of Mount St. Elias. Here numerous ice-rivers meet and unite in one great sheet about 1,500 square miles in extent. Mr. Russell states that the glaciers of Alaska are slowly retreating, and that this recession has probably been going on steadily for the past 100 or 150 years.

Mr. Russell points out that Alaska is not so densely forested as is generally assumed,—and that, in fact, the tree region is confined to the south-eastern part of the country, most of the western portion being treeless. The article concludes with some observations on the inhabitants, and will well repay perusal as a general summary of information on a region at present but little known.

WE have received from M. Augusto Nobre the number for July, 1894, vol. i., no. 3, of that enterprising Portuguese publication, the *Annaes de Sciencias Naturaes*. Among other interesting matter it contains a

plate representing a bizarre-looking Selachian, captured at Buarcos. Mr. G. A. Boulenger, of the British Museum, who was consulted, refers it to *Squalus rostratus*, Marc, which Dr. Günther identifies with *Selache maxima*. Dr. Lopes Vieira, who gives an account of the specimen, is as yet by no means satisfied with the identification. From the figure and details given we are inclined to agree that if it is *Selache maxima* it is a very abnormal specimen.

In the last number of the *Journal of Anatomy and Physiology*, Mr. F. G. Parsons describes the tendo achillis of mammals, as being formed by a rope-like twisting of the fibres derived from the two heads of the gastrocnemius, the soleus, and the plantaris. The twisting which can be made out in man he regards as being partly due to the rotation of the limb in the embryo, but more largely to the position of the foot in the uterus of the mother.

Dr. A. Grandidier has issued two more volumes of plates illustrating the natural history of Madagascar. These are the fourth and fifth volumes of "Histoire Naturelle des Plantes," By H. Baillon. Once more we regret to observe that the plates are unaccompanied by any text or explanation.

In the *American Journal of Science* for August, 1894, Mr. O. L. Simmons, of Tufts College, Mass., writes on the "Development of the Lungs of Spiders." He comes to the conclusion that "the lung-book of the spider, and presumably of all arachnids which possess one, arises at first as an external structure upon the posterior surface of the abdominal appendages. These appendages sink in without any inversion or complications, in exactly the manner theoretically deemed probable by Kingsley, so that there can no longer be any doubt as to the exact homology existing between the lungs of the spider and the first pair of gills in Limulus."

The beautiful little fresh-water alga *Volvox* is well-known to lovers of ditch and pond life. We see from the July number of *The Botanical Gazette* that Mr. W. F. Shaw has found in a ditch at Palo Alto, California, an allied plant, which he considers to be and describes as a new genus, *Pleodorina*. The name indicates its affinity with *Eudorina*. It resembles *Volvox* more than does the latter genus in the number of cells composing the individual and the specialisation of certain cells for the purpose of reproduction. No sexual generation has, however, yet been observed.

OBITUARY.

GEORGE HUNTINGTON WILLIAMS.

BORN 1856. DIED JULY 12, 1894.

ONE of the most prominent members of the new school of Petrography in America has been prematurely removed in the person of Dr. G. H. Williams, Professor of Inorganic Geology in the Johns Hopkins University, Baltimore. Though only in the prime of life, he succumbed in July to an attack of typhoid fever; his health, it is supposed, having been enfeebled by his arduous duties last summer in the department of Mineralogy of the Columbian Exposition. Professor Williams was a native of Utica, N.Y., and graduated at Amherst College in 1878. He subsequently resided for a short time in Berlin, and then studied under Rosenbusch in the University of Heidelberg, where he obtained the degree of Ph.D. in 1882. The following year he became connected with Johns Hopkins University, and was Associate Professor there from 1885 to 1892, when he was appointed to the Chair he held at his death. The Professor was an attractive teacher, and numbered among his pupils many of the most successful workers in Petrography of the rising generation in America. Apart from his numerous original memoirs and the "Geology of Maryland," which we reviewed some months ago, his well-known "Text-book of Crystallography" will long keep his memory in the mind of students; and it is a distinct loss to science that his projected work on the microscopic structure of American crystalline rocks, in progress at the time of his death, should have been thus abruptly terminated.

THE death of M. DUTREIL DE RHINS has robbed France of one of her best geographers. M. de Rhins had reached Tibet in course of a journey from Turkestan to China and was murdered during a dispute over some horses. It is a satisfaction, though a poor one, to learn from the newspapers that the Chinese Government have promptly ordered the recovery of his papers and effects, and further suggested some reparation to his family. M. de Rhins was only forty-seven, but he has left a monument behind in his account of the geography of Central Asia.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

PROFESSOR Dr. E. RATHAY has become Director of the "önologisch-pomologischen Lehranstalt" at Klosterneuburg, near Vienna, and Dr. C. Rechingen provisory assistant to the botanical chair at Vienna. Dr. René Koehler has been appointed Professor of Zoology to the Faculty of Sciences of Lyon, and Mr. Wm. B. Marshall, of the New York State Museum, Professor of Biology in Lafayette College, Easton, Pennsylvania. The Kansas State University has divided the subject of Geology between two chairs, giving stratigraphical and physical geology to Professor E. Haworth, and leaving palæontology in the hands of Professor Williston.

DR. WILLIAM KING, the late Director of the Indian Geological Survey, is on his way home to England. Mr. C. L. Griesbach has been temporarily placed in charge, and as he is the senior official, it is probable that he will succeed to the vacated post.

MR. PETER ESSLEMONT, the head of the Scottish Fishery Board, has just died. It is to be hoped that his successor will have a competent scientific acquaintance with the matters with which he has to deal. No more suitable successor could be found than Mr. John Murray, of the "Challenger." We understand that strong representations in his favour have already been made by a large number of scientific men, and we need do no more than endorse their recommendation.

DR. FELKIN has been appointed to represent the Royal Scottish Geographical Society at the meeting of the German Congress of Naturalists and Physicians, to be held in Vienna during the month of September.

DR. JULIUS WIESNER has been receiving the congratulations of his past and present pupils on the completion of the twenty-fifth year of his professorship, the twentieth year of his presidency over the Institute of Plant Physiology of the Vienna University, founded by himself, and, thirdly, on the occasion of his safe return from a long journey of exploration to Java. The Professor was presented with an address and a silver medal of great artistic value.

PROFESSOR DR. PFITZER, of Heidelberg, has been raised to the dignity of Geheim-hofrath. A committee has been formed in Gera to erect a monument to the deceased ornithologist, Karl Theodor Liebe. The Zoological Garden of Berlin, having reached its jubilee, proposes to erect a monument to the energetic director, Dr. Heinrich Bodinus. Subscriptions may be sent to Mr. E. Weigel, 141 Frankfurterstrasse, Berlin, O.

A CURATOR is wanted for the Museum of the Institute of Jamaica, at Kingston, Jamaica. The appointment will be for three years, and is renewable at the end of that period. Applications should be addressed to the Secretary of the Institute of Jamaica, care of Messrs. H. Sotheran & Co., 140 Strand, London, W.C., not later than October 15, and should be marked "Curator, Institute of Jamaica." Further information may be obtained from Messrs. Sotheran. The salary offered is at the rate of £400 a year.

THE Swiney Lectures on Geology, under the auspices of the trustees of the British Museum, will this year again be delivered by Professor H. Alleyne Nicholson, who will take for his subject "The Making of the Earth's Crust." The lectures, which are on Mondays, Wednesdays, and Fridays, from the 1st to the 26th of October, will be delivered in the Lecture Theatre of the South Kensington Museum, since the proposed and much-needed lecture theatre of the Natural History Museum is still *in nubibus*. We further understand that this is the last year, for the present, in which the post will be filled by the very popular lecturer, Dr. Nicholson. For the ensuing year, at least, the well-known anthropologist, Dr. J. G. Garson, has been appointed Swiney Lecturer, and is expected to deal with the Geological History of Man. Meanwhile the Natural History Museum at Torquay is to have its Lecture Theatre built out of the £1,360 subscribed as a memorial to the late Mr. W. Pengelly.

THE new Norwich Castle Museum, of which we published a detailed description (vol. i., p. 692), is now nearly completed, and the removal of the collections is in active progress. The formal opening, by the Duke of York, is announced to take place next month.

NEXT month the local collections of the Natural History Society of Tromsö, Norway, will be removed from the present inconvenient house to a fine new stone building, which has been occupied during the summer by the Tromsö Centenary Exhibition. The Museum is of two storeys, with convenient work-rooms and an attic for stores, and has been built by the town and district, aided by a Government grant. The Tromsö Society, though small, displays remarkable vigour and has formed one of the finest collections of a local fauna and flora to be met with in any Continental Museum. It has also obtained a large series of fossils from Spitzbergen and Novaya Zemlya.

MR. A. HALY, the Director of the Colombo Museum, Ceylon, has issued his report for 1893. The chief object of this museum is to give a full representation of the products of Ceylon, both natural and artificial, and Mr. Haly points out how necessary it is that such a collection should be accompanied by a complete collection of the literature relating to Ceylon. He does not, however, find much value in the "Descriptions of Ceylon species published in various scientific periodicals." "In most cases," he says, "the descriptions of insects are unaccompanied by any figures, and are quite useless for the purpose of naming the species described, apart from authoritatively named specimens. *Entomologists, please note!* This museum has recently acquired very fine specimens of the Dugong—male, female and young. The female is 11 feet long, and is said to be the largest ever seen. A new kingfisher has been found on the island, apparently allied to the S. Indian species, *Alcedo bavani* of Walden. A new starred tortoise has also been found, which appears to be peculiar to Ceylon. A fish of the genus *Duymaeria*, is the first representative of that genus recorded from the Indian Ocean. A process of preservation by gum and glycerine is now used for fish, crustaceans, starfish, and holothurians. Instead of cork for insect boards, Mr. Haly uses a mixture of sawdust and cocoa-nut fibre.

MR. ZENKER, head of the station at Taunde in the Cameroons, has sent a large collection of bird-skins to the Royal Zoological Collection in Berlin. Dr. Stuhlmann also has sent many interesting novelties from Usaramo, in East Africa. Both these collections will be described in the *Journal für Ornithologie*. *A propos*, we learn that Dr. A. Reichenow's work "Die Vögel Deutsch-Ostafrikas" will be published in the autumn of this year by Dietrich Reimer, in Berlin.

THE Penzance Natural History and Antiquarian Society has issued its *Report and Transactions* for 1893–94. The most important paper is a critical list of the Lepidoptera of Cornwall and the Scilly Islands, by the President, Mr. W. E. Baily. Many local lists have already been published, but this is a more general summary

and includes the results of the author's own work during the last ten years. The Cornish Lepidoptera, as a rule, are larger than their congeners in other parts of Britain. Among brief notes, it is announced that a fine example of Risso's Grampus *(Grampus griseus)* was stranded near Marazion in April last, and that the skeleton is being prepared for description in the next part of the Society's *Transactions*. Details of the external characters of the animal are given in the present note. Meteorological observations used to be made at Penzance by Dr. Giddy, but no regular records were made between 1887 and 1893. The Corporation have now instituted systematic observations for the borough, and hope to bridge over this gap from private sources.

THE Malton Field Naturalists' and Scientific Society has commenced the issue of a monthly record of observations on local Natural History, entitled *Naturalist Notes*. This is one of the most vigorous of provincial societies, and has already formed a museum in many respects unique; but we doubt the wisdom of its latest venture, which strikes us as an unfortunate substitute for a well-digested annual report. At the recent annual meeting Professor L. C. Miall retired from the Presidency, and was succeeded by Mr. A. S. Woodward, of the British Museum.

THE Norfolk and Norwich Naturalists' Society has issued its transactions for 1893-94—vol. v., part 5. Mr. C. B. Plowright, M.D., has been appointed president for 1894-5. The retiring president, Mr. T. Southwell, F.Z.S., whose address appears in the present number of the *Transactions*, took for his subject the past and present history of the society, which is now in the twenty-fifth year of its existence. Many important and interesting contributions appear in the number, and there is issued with it a photograph of a specimen of the tropic bird *Phaëton athereus* found dead at Cradley, Herefordshire.

THE Midland Union of Natural History and Scientific Societies held its seventeenth annual meeting at Ellesmere, Salop, on Friday and Saturday, August 3 and 4. The proceedings commenced with the council meeting on Friday afternoon, and eleven delegates from the various societies in the Union attended. The report of the executive committee was received, and after considerable discussion adopted. It was decided among other items that papers competing for the Darwin Medal should be published within the area of the Union, and that the Union should be made primarily a record office for the Natural History of the Midlands.

The new executive committee consists of the secretaries or a representative of each society in the Union, and six elective members as follow: Professors Allen, Bridge, and Lapworth of Mason College, Messrs. Chase, Hughes, and Wilkinson, with Professor Hillhouse and T. V. Hodgson as secretaries.

The annual meeting was held later, and accepted the recommendations of the council; the other officers were unanimously elected, viz., Brownlow R. C. Tower as president, and E. de Hamel as treasurer. It was also decided to admit photographic societies into the Union.

A conversazione was held in the evening at St. Oswald's College, and attended by upwards of 200 members and friends. There was a fair number of exhibits, chiefly local, and the college museum was thrown open. One paper was read by Mr. Bellamy, of Oxford, on the Migration of Birds.

Saturday was taken up by two excursions, one for archæologists to Chirk Castle and Valle Crucis, Llangollen, conducted by Mr. A. T. Jebb; the other for biologists, round the meres and peat mosses of the neighbourhood, led by Messrs. H. J. E. Peake and J. A. S. Jennings. Both excursions were well attended and very successful, notwithstanding the heavy rain which came down in the afternoon.

The botanists succeeded in finding most of the local plants, including *Nuphar pumila*, *Lobelia Dortmanna*, *Alisma natans*, *Utricularia minor*, etc., etc. The meres were all found to be "breaking," Ellesmere and Whitemere with *Glœotrichia echinata*, Colemere and Crosemere with *Aphanizomenon flosaqua*; in

spite of this *Leptodora hyalina*, *Daphnia jardinii*, and *Bosmina coregoni* were taken. The two latter do not appear to have been recorded from this district before.

Several members of the Union prolonged their visit over the Bank Holiday, and were able to make a more exhaustive search in certain areas with good results.

The hospitality and generosity of the Ellesmere people were unlimited, and afforded striking evidence of the capacity of a small local society.

DURING the week ending August 11 the members of the Geologists' Association visited Shropshire on their long excursion, under the direction of Professor Lapworth and Mr. W. W. Watts. The headquarters during the whole visit were at Shrewsbury. On Sunday, July 29, many of the members visited the Breidden Hills. During the rest of the week the programme was as follows:—Monday, Visit to the Wrekin, under the direction of Dr. Callaway; Tuesday, Visit to the Longmynd with Professor Blake; Wednesday, the eastern portion of the Shelve area, under the direction of Messrs. Lapworth and Watts, who were also the leaders on the following day, when it was arranged to visit the more westerly parts of the same district and to see the laccolite of the Corndon. Unfortunately heavy rain delayed the start, so that the former part of the programme had to be omitted. On Friday the famous Onny Section was examined, under the guidance of Professor Lapworth and Rev. J. D. La Touche. On Saturday morning the Mayor of Shrewsbury received the members at the Museum, which is of considerable interest. Later in the day Buildwas Abbey and the Severn Gorge at Coalbrookdale were visited, and the excursion brought to a close.

A valuable pamphlet on the "Geology of Southern Shropshire," by Lapworth and Watts, was issued to the members, and will shortly be published in the *Proceedings* of the Association.

WHILE we are finding coal and ironstone at Dover, and while Professor Boyd-Dawkins is advocating a search for coal in Oxfordshire, arguments similar to those employed by English geologists for England have led the geologists of New South Wales to the discovery of coal under Cremorne in Sydney Harbour. Of late years the existence of coal under Sydney has been regarded by local geologists as almost a certainty, the only divergence of opinion being about the depth at which it was likely to occur. A seam of coal has now been found by boring in the above locality, at a depth of 2,801 and 2,917 feet, which varies in thickness from 7 feet 3½ inches to 10 feet 3 inches, and contains only minute bands of shale. The coal is good and especially suitable for steaming purposes. An account of the various borings is given by Messrs. T. W. E. David and E. F. Pittman in the *Records of the Geological Survey of New South Wales*, vol. iv., pp. 1–7.

AT a meeting of the Linnean Society of New South Wales on June 27, Mr. Hedley exhibited the shelly tubes of *Kuphus arenarius*, collected by Mr. Brazier on a coral reef at Florida Island, Solomon Islands. As the affinities of this animal are somewhat problematical, visitors to the Solomons should endeavour to procure it.

PRINCE HENRI D'ORLEANS arrived at Tamatave some weeks ago, and proceeded north to the place where Mr. Muller, the French explorer in Madagascar, met his fate a few months ago.

THE library of Professor Carl Vogt, of Geneva, has been purchased by the Government of Roumania.

MR. FRANK FINN, M.A., F.Z.S., has been appointed First Assistant-Curator in the Zoological Department of the Indian Museum at Calcutta.

DR. HENRI FILHOL has been nominated to the chair of Comparative Anatomy at the Natural History Museum, Paris, lately vacated by the death of Mr. Georges Pouchet.

CORRESPONDENCE.

The Art of Cataloguing.

Sir,—Whoever reviewed Mr. Seward's Catalogue of the Mesozoic Plants in the British Museum in your August number (p. 147) is no doubt a competent botanist, but can know very little about museum work. In cataloguing a series of fossils, whether plants or animals, "a bare quotation of the register numbers," would by no means serve the purpose, though your reviewer seems to think it would. In the first place, register numbers have an unfortunate habit of coming off, and it is exceedingly useful to have some subsidiary means of identifying the specimens. Secondly, a register number alone conveys no information to a worker at a distance; and it is surely advisable that such an one should be informed whether a specimen is a mere fragment, or shows some structural features, so that when he comes to the Museum he may be able to say at once exactly which specimens he wishes to see. At the worst, the system adopted by Mr. Seward, as by some other cataloguers before him, can do nothing more terrible than take up the space of which your reviewer seems so jealous; and I venture to submit that the space is by no means wasted.

Geological Department,
British Museum.

F. A. Bather.

Nascent Elements.

With regard to the question of nascent elements, has it been considered whether the conditions which apply to several gases in the above state also apply to them when solid, *i.e.*, if hydrogen could be evolved under conditions consistent with a solid state, what peculiarities different to solid hydrogen, evolved and then solidified, would it exhibit? Also when, say, carbon is freed from a compound does *it* exhibit any nascent phenomena?

Would not a solution of this problem shed light on the supposed action of chlorophyll in the formation of starch from CO_2 and H_2O in plants?

Hoping you will discuss this question in your valuable magazine.

Jno. E. Hardman.
E. P. Greenhalgh.

78 Chorley Old Road, Bolton.

[Whatever may be the conditions requisite for the evolution of solid hydrogen, those existing in the leaves of living green plants must be so vastly different as to render comparison useless. As regards carbon, it is extremely improbable that the free element enters at all into the process of formation of organic matter from carbon di-oxide and water; it is generally supposed that the CO_2 loses only half of its oxygen, the remaining group (CO) combining with the hydrogen of the water to form a compound containing the three elements in these proportions (CH_2O). This compound is a much simpler one than starch, which is only deposited by the protoplasm when the amount of carbohydrate material present in the cell is in excess of that needed for the immediate use of the plant.—Editor.]

Natural Selection.

In answer to Professor Henslow and "Entomophilos," I may perhaps state that the various facts on which my statements were based are contained in my own and other papers, to which I must refer them. The word "apparently" in the phrase they object to might be replaced perhaps by "possibly" or "probably."

Natural Selection, like Inheritance of Acquired Characters, is a convenient working hypothesis, and has still to be *dis*proved.

The first part of Entomophilos's letter I do not follow. By the statement I made I did not intend to imply more than a resemblance between our flora and those of Northern Europe. It has been abundantly shown that the circle of visitors to a given flower is of a very definite nature, and from the fact that in our fauna there is a "natural preponderance of flies over other insects," it does not follow that there will be a similar preponderance of flies among the insect-visitors of certain flowers.

JOHN C. WILLIS.

DISCUSSIONS AT THE BRITISH ASSOCIATION.

I WAS present at the discussion on Natural Selection in the Subsection of Zoology, and feel strongly that the method adopted was not that most calculated to lead to useful result. After a number of interesting objections to Natural Selection had been urged, the most notable defenders of Darwinism contented themselves with implying either that their adversaries did not understand what they were talking about, or that they were unacquainted with the writings of Darwin. I wish to support the judicial proposal of Sir Edward Fry, that these discussions should be conducted like a case in a court of law, and that the counsel on either side should be kept to the point at issue. I venture to suggest that, instead of lumping together a somewhat heterogeneous set of papers, and then taking a discussion on the lot, it would be well for the secretaries of the organising committees to settle beforehand on definite subjects, or, better still, on definite propositions, and then to get specialists to contribute arguments from their own knowledge on either side of the case

GALLIO.

CORRIGENDUM.

C. H. B. WOODD, of Roslyn, Hampstead, calls our attention to an unfortunate slip in the July number. NATURAL SCIENCE, vol. v., p. 16, 6th line from bottom of page; for "were interchangeable terms," read "were not interchangeable terms."

TO CORRESPONDENTS.

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